# Abstract

The Affect Misattribution Procedure has attracted considerable attention and use in psychological science as a measure of evaluations, attitudes, and biases. The AMP’s appeal to researchers is based in large part on the promise that it taps into unintentional and unaware (i.e., implicit) psychological processes. However, past claims about the implicitness of AMP effects may be inaccurate due to a range of methodological, statistical, and conceptual issues. We re-examine a key premise underpinning the AMP’s use (i.e., that AMP effects are driven by the *unaware* *influence* of primes on responses). Across five pre-registered experiments (*N* = 1021) plus meta-analyses, we demonstrate that AMP effects and their predictive validity are primarily driven by a subset of influence-aware trials (within individuals), and high rates of influence-awareness (between individuals). Counterintuitively, an individual’s influence-awareness rate in one AMP predicts their performance in a previously completed AMP, even when the AMPs assess entirely different attitude domains*.* Taken together, our results suggest that AMP effects are not particularly implicit, are not mediated by misattribution, and furthermore do not represent an equally valid measure of attitudes across individuals. All materials and data available at [osf.io/gv7cm](https://osf.io/gv7cm/).

*Keywords*: Affect Misattribution Procedure; automaticity; implicit social cognition; implicit measures

The AMPeror’s New Clothes: Performance on the Affect Misattribution Procedure is Mainly Driven by Awareness of Influence of the Primes

Over the last twenty years research on implicit cognition has exploded from a relatively small area into, what is today, one of the most prolific and widely examined topics in psychological science. The idea that our automatic thoughts, feelings, and actions shape downstream behavior drives research, theory, and application throughout the discipline, particularly in social and personality psychology, neuroscience, health, cognitive, and clinical psychology (for a book length treatment see Gawronski & Payne, 2010).

The success of the topic has in large part been due to the development and widespread use of procedures known as *indirect measurement procedures*. In contrast to *direct measurement procedures*, which simply ask people to directly report their thoughts, feelings, and actions, indirect measurement procedures seek to indirectly probe the mind by interpreting participants’ performance (e.g., speed and/or accuracy) on experimental paradigms. The outcomes of these measurement procedures are commonly referred to as *implicit measures* (De Houwer, 2006). Notable examples include the Implicit Association Test (IAT: Greenwald, McGhee, & Schwartz, 1998), evaluative priming tasks (Hermans, De Houwer, & Eelen, 1994), and approach-avoidance tasks (Rinck & Becker, 2007; for a review see Gawronski & De Houwer, 2014).

Indirect measurement procedures are often deployed under the assumption that they limit a person’s ability to control how they respond, as well as the need for them to introspectively access, or be consciously aware of, the content under investigation. As a result, implicit measures have historically been used whenever researchers want insight into content that people may be unwilling or unable to report (see Greenwald et al., 1998; Hahn & Gawronski, 2019). Although debate continues about what implicit measures actually capture (Brownstein, Madva, & Gawronski, 2019; Corneille REF; Schimmack, 2019), a vast and ever-increasing number of studies continue to rely on them to provide insights that self-reports cannot.

## The Affect Misattribution Procedure

The Affect Misattribution Procedure (AMP) has emerged as one of the more popular indirect measurement procedures because it possesses the structural advantages of sequential priming along with good psychometric properties (which priming and other reaction-time tasks often lack: see Payne & Lundberg, 2014). At its core, the AMP consists of a series of trials that are themselves comprised of three main components: a prime image (e.g., a racial group member) which is first flashed on screen for a brief period of time, followed quickly by a target image (usually a neutral Chinese symbol), which is subsequently masked by a white noise image. The AMP requires participants to subjectivelyevaluate how visually pleasing the ambiguoustarget image (Chinese character) is, while ignoring the prime image which preceded it. Despite being explicitly told to disregard the prime when evaluating the target, people nonetheless evaluate the target image in ways that are consistent with the valence of the prime that preceded it. For instance, when a neutral Chinese character is preceded by a social in-group member, people are more likely to evaluate the Chinese character as pleasant than if it is preceded by an out-group member (Payne, Cheng, Govorun, & Stewart, 2005).

**Two Competing Accounts of the AMP Effect**

The AMP receives this extensive use in psychology and beyond because it is assumed to meet two conditions. Firstly, the AMP is assumed to be a valid measure of evaluations: researchers assume that effects in the AMP are primarily the result of the evaluations of the participants towards the stimuli used within the task. Secondly, the AMP is assumed to represent an *implicit* measure of evaluations: specifically, that behavior within the task is emitted without an intention to do so, and without awareness of the influence of the primes on the responses of participants[[1]](#footnote-1). With regard to this latter point, two distinct perspectives on the AMP are present in the literature. On the one hand, many users of the AMP align with an *implicit account* of AMP effects: namely, that effects in the AMP indeed reflect behavior which is captured without the intention or awareness of the participant (Payne et al., 2005; Payne et al., 2013). On the other hand other researchers have opposed this perspective, and instead forward an *explicit account* of AMP effects which argues that participants are both aware of the influence of primes in the AMP, and exert intentional control over their behavior in order to respond in-line with these primes (Bar-Anan & Nosek, 2012; Mann et al., 2019).

The importance of resolving whether AMP effects are better explained by an implicit account or an explicit account for the future use of the measure cannot be overstated. The AMP’s raison d'etre in most studies relates to its implicitness, rather than only its validity as a measure of evaluations: if AMP effects do not reflect unintentional and unaware responding, then most researchers would likely simply opt for briefer, more straightforward self-report measures to assess evaluations. This would have a profound impact on when and how the AMP is used within psychological research. Fortunately, researchers have appreciated the urgency of this matter and conducted a number of investigations into this matter. Much of this research has focused on the (un)intentional nature of AMP effects. Unfortunately, the results of these investigations have been somewhat inconclusive: some have found support for the unintentionality of AMP effects (Payne et al., 2005, 2013; Gawronski & Ye, 2013), while others have found that AMP effects are largely intentional (Bar-Anan & Nosek, 2012; Mann et al., 2019).

**Awareness in the AMP**

Unlike (un)intentionality, the (un)aware nature of AMP effects has received comparably little attention. This is in spite of the fact that capturing evaluations that occur outside of awareness is a critical motivator for researchers using the AMP (REFs), and many would even argue that unawareness is a necessary precondition for unintentional responding to occur (REFs). One study has specifically addressed the issue of awareness in the AMP. In their third experiment, Payne and colleagues (2013) divided participants into two groups. The first group completed a traditional AMP, whereas the second completed a modified AMP containing three response options: people could (a) indicate that the target stimulus was pleasant, (b) indicate the target stimulus was unpleasant, or (c) ‘pass’ that trial if they felt that their evaluation would be influenced by the prime. The authors argued that if AMP effects were due to responding on trials where participants were aware of the influence of the prime on their evaluations of the targets, then allowing for these trials to be skipped “should eliminate the priming effect” (p. 377).

In support of the implicit account of AMP effects, the authors found that allowing people to skip a trial when they felt their target evaluation would be unduly influenced did not lead to significant changes in the magnitude of AMP effects. Put another way, an AMP comprised solely of ‘uninfluenced’ responses still produced meaningful outcomes. This was offered as evidence that AMP effects occur when people are unaware of the prime’s influence on their evaluative behavior. Although a promising starting point, this experiment had two critical issues which limit the inferences which can be drawn from it. Firstly, tSecondly, because participants were required to *either* evaluate the prime *or* skip the trial if they felt they were influenced by the prime, the authors did not collect evaluative responses for influence-aware trials. A direct comparison between the magnitude of effects in influence-aware vs. non-influence-aware trials within AMPs and within participants (rather than comparisons of AMP effects between different AMPs completed bydifferent groups) would be more informative regarding the (un)awareness of AMP effects.

**Validity of the AMP**

Although both the implicit and explicit accounts of AMP effects disagree on the *conditions* under which responding in the AMP occurs, they generally agree that the AMP is a *valid* measure of evaluations. This is likely an uncontroversial assumption: the AMP appears to have predictive utility in a number of domains (REFs), exhibits good convergent validity with self-report measures (REFs), and is generally known as one of the most psychometrically reliable implicit measures (REF). Indeed, the AMP procedure *in general* appears to be a valid measure of evaluations. At the same time, it is increasingly clear that the AMP is not a process-pure measure: explicit responding (either aware, intentional, or potentially both) is present in a nontrivial proportion of trials in the AMP (Payne et al., 2010, 2013; Mann et al., 2019). As such, an additional question may now be asked: is the AMP a valid measure of evaluations on those trials which can be considered implicit? Although there are studies which are relevant to this question in the context of unintentionality (e.g., REFs), there has been little work addressing this question in the context of unawareness. At a time when the validity of many psychological measures has been cast into doubt (Hussey & Hughes, 2020; Flake et al., 2017; Flake & Fried, 2019; Yarkoni, 2019; REFs), addressing this question is critical to determine how, when, and where the AMP will be used in the future.

## The Current Research

With the questions of the (un)awareness and validity of AMP effects in mind, we carried out seven pre-registered, highly powered experiments that sought to determine if participants are aware of the influence of prime stimuli on their AMP performance, if AMP effects are predominantly seen on aware or unaware trials, and whether the validity of the AMP as a measure of evaluations is applicable to aware trials, unaware trials, or both. To answer this question, we adapted the ‘skip’ paradigm used by Payne et al. (2013, Experiment 3) and refined it in several ways. First, we employed within- rather than between-subjects designs in order to increase the power of our analyses. Second, we sought to overcome the statistical issues present in past work. Specifically, in cases of non-significant findings we pre-registered the use of Bayesian statistical methods in order to assess the weight of evidence for statistical equivalence. In this way, we ensured any inferences made about statistical equivalence were supported by the appropriate statistical methodologies. Third, we assessed awareness of influence in such a way that we could analyze participants’ evaluations on both non-influence-aware *and* influence-aware trials. In Experiments 1-5, we did this by asking participants whether they were aware that their responses were influenced by the prime stimulus *immediately* *after* each individual trial.

On the basis that aware responses in this task may be the consequence of post-hoc confabulations (i.e., that participants are not aware of the influence of the prime when initially giving their evaluative response, but then notice upon reflection that their response was consistent with the valence of the prime, and thus respond that they were influenced), we varied the position of our influence-awareness question in Experiments 6 and 7. In Experiment 6, we asked participants *before* they elicited their evaluative response whether they *would be* influenced by the prime when giving their response. In Experiment 7 we pushed this even further, and asked participants whether their evaluation of the target would be influenced by the prime beforethey saw the target stimulus (see Figure X for an overview of the seven experiments). Following these seven experiments, we conducted meta-analyses of three hypotheses that were present across the experiments. Specifically, we meta-analyzed effects to determine whether (1) IA-AMP effects are driven by influence-aware trials, (2) between participants, IA-AMP effects are driven by highly influence-aware participants, and (3) influence-awareness rates in the IA-AMP are also predictive of effect sizes in a previously-completed traditional AMP.

In all seven Experiments, we assessed the question of awareness within the AMP by investigating whether the prime-consistent responding varies between trials which are influence-aware vs. non-influence-aware, and whether AMP effects between participants correlate with increasing rates of influence-awareness. In addition, we investigated the validity of influence-aware vs. non-influence-aware trials and AMP effects in a number of ways. Our fourth Experiment assessed the relative validity of AMP effects produced by influence-aware vs. uninfluence-aware trials in terms of known-groups prediction. We additionally meta-analytically compared the split-half reliabilities for AMP effects produced by influence-aware trials vs. non-influence-aware trials. We also tested a foundational assumption of the measurement model implied in the calculation of the AMP effect: that evaluations in trials with one prime type and evaluations in trials with another prime type should be negatively correlated (or at the very least, not significantly positively correlated).

Across these seven experiments and meta-analyses, we find consistent evidence that (i) prime-consistent responding is seen primarily in influence-aware trials, (ii) AMP effects are strongly correlated with influence-awareness rates, (iii) influence-awareness rates in one AMP predict the size of the AMP effect in another AMP, *even when the two AMPs assess* *different domains*. We also find that the split-half reliabilities are larger for influence-aware AMP effects than non-influence-aware AMP effects, and that the AMP strongly predicts known-group membership when using influence-aware AMP effects, but only very weakly when using non-influence-aware AMP effects. Perhaps most saliently, we find that the AMP conforms to its implied measurement model only when the majority of trials in participant performances are influence-aware. Overall, we present a wealth of evidence which suggests that responses in the AMP which are valid are not implicit (in the sense of unaware), and that responses in the AMP which are implicit are not valid.

# Experiment 1: IA-AMP effects are Driven by Awareness of the Prime’s Influence

In all experiments in this paper, we report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study (Simmons, Nelson, & Simonsohn, 2012). In Experiment 1 we set out to answer the following questions. First, are (IA-)AMP effects driven by a subset of trials; namely, those in which participants report that their evaluation of the target was influenced by the prime stimulus? Second, are (IA-)AMP effects driven by participants who are more often aware of the prime’s influence? Third, does our on-line measure of awareness correlate with the post-hoc self-report measure of awareness typically used in this literature? Fourth, does this online awareness measure predicted AMP effects to a greater extent than the self-report awareness measure? Our primary pre-registered hypothesis was that the (IA-)AMP effect (both the trial- and participant-levels) would be primarily driven by awareness of prime influence. We also expected that the online and self-report measures of awareness would be related, but that the online awareness measure would be a better predictor of (IA-)AMP effects than its post-hoc counterpart.

## Method

For all experiments in the current manuscript, ethical approval was provided by the Ethical Committee of the Faculty of Psychology and Educational Sciences at XXXXX University (approval numbers 2015/13, 2016/63, and 2016/80). Written consent was obtained from all participants prior to completion of all experiments.

Research materials for all experiments in this paper can be found on the Open Science Framework ([osf.io/gv7cm](https://osf.io/gv7cm/)). This includes details of the design, Inquisit scripts for the measures, raw and processed data, analytic plans, and all R code for data processing and analysis. Data for all experiments were collected online via the Prolific platform ([prolific.ac](http://prolific.ac)). The pre-registration for this experiment is available at [osf.io/p6e3c](https://osf.io/p6e3c/).

**Participants.** Based on power analyses (i.e., 95% power to observe a medium effect size [*f*2 = 0.15] in a linear regression analysis with a single predictor at the 0.05 alpha level) and available resources, our a priori sample size after exclusions was 150 participants. Our sampling strategy involved initially recruiting 150 participants and then, upon excluding participants based on partial or incomplete data or meeting any of the exclusion criteria below, adding further participants in batches of 10 until we had analyzable data for at least 150 participants. Analyses were conducted on the final analytic sample only, once the predetermined sample size had been reached. 214 total participants took part and were paid £1.25 for completing the study. Completed data from 153 participants (67 men, 86 women) was collected, with an age range from 18 to 65 years (*M* age = 34.73, *SD* = 11.70).

**Materials.** All measures were programmed in Inquisit 4.0 and administered via the Inquisit Web Player. The influence-awareness Affect Misattribution Procedure (IA-AMP) consisted of a near-identical layout and stimulus set as the AMP used in Experiment 3 of Payne et al. (2013). The primes consisted of 12 positive and 12 negative images taken from the IAPS (Lang, Bradley, & Cuthbert, 1997). Our IA-AMP differed from Payne et al.’s AMP in three ways: (a) our IA-AMP consisted of 120 rather than 72 critical trials, (b) no neutral primes were used (consistent with most AMP research; e.g., Gawronski & Ye, 2015), and (c) rather than allow participants to skip trials if they felt that they would be influenced by a prime, we instead asked them to respond to every trial (i.e., “Press spacebar if the picture influenced your response to the Chinese symbol”), and thereafter indicate if that response was influenced by the prime (i.e., by pressing the spacebar during a fixed 2000ms post-response interval). This variation allowed for within-subject comparisons of influenced vs. non-influenced trial performance.

**Procedure.** Participants first provided informed consent, then completed demographic information (age and gender) followed by the IA-AMP. Thereafter they completed a self-report awareness of prime influence question. This question was identical to that of Payne et al. (2013) (Experiment 1). Specifically, participants were asked: “to what extent were your ratings of the Chinese symbols influenced by the pictures that appeared immediately before those symbols?”. They responded using on a 7-point Likert scale (“Never”, “Very rarely”, “Somewhat rarely”, “Sometimes”, “Somewhat often”, “Very often”, “Almost always”).

## Results

**Analytic Strategy**. For investigating questions relating to the presence of an IA-AMP effect in general, we employed logistic mixed-effects models. To investigate the role of prime influence-awareness on performance in the IA-AMP at the trial-level, we also utilized logistic mixed-effects modeling. The use of mixed-effects models in general provides far superior statistical power compared to their more commonly used fixed-effects alternatives. Therefore, logistic mixed-effects models provide us with an opportunity to maximize power where possible. To address the extent to which influence-awareness informed IA-AMP scores at the participant-level, we scored IA-AMP effects for each participant (see below) and entered these into linear regression models. Finally, to compare online and offline measures of influence-awareness, we utilized both correlational and regression analyses. All reported analyses were pre-registered unless noted otherwise.

**Data Preparation**. In order to participate, participants were required to be between the ages of 18-65, have fluent English, an approval rating of greater than 90% on Prolific Academic, and to not have participated in similar studies by our research group. Additionally, we specified a number of exclusion criteria: if participants completed the experiment in under 3 minutes, or provided incomplete data in any of the measures, they were excluded from subsequent analyses. In analyses at the participant-level, we computed IA-AMP effects for participants using the standard method (i.e., subtracting the proportion of ‘pleasant’ responses to trials which included an unpleasant prime from the proportion of ‘pleasant’ responses to trials which included a pleasant prime: Payne et al., 2005). Given that we were interested in the *magnitude* of participants’ IA-AMP effects, regardless of their directionality, all analyses using participant-level effects use the absolute value of IA-AMP effect (i.e., the difference in evaluations between the prime types, agnostic to the direction of the effect). In addition, we also calculated influence rates for each participant in the IA-AMP by dividing the number of trials where participants reported having been influenced by the prime (i.e., by pressing the spacebar) by the total number of trials in the IA-AMP.[[2]](#footnote-5)

**Hypothesis Testing**.

*Do we find evidence for an IA-AMP effect?* We first sought to verify that an IA-AMP effect emerged. To do so we carried out a logistic mixed-effects model, with Valence Ratings of Chinese characters in each trial (pleasant or unpleasant) as the dependent variable, Prime Valence (pleasant or unpleasant) as the independent variable, and Participant as a random effect. This served to acknowledge the non-independence of the multiple data points provided by each participant (i.e., the hierarchical nature of the data). Results revealed evidence of an IA-AMP effect as expected, such that participants were more likely to rate Chinese characters as positive when the prime valence was positive compared to when the prime valence was negative, OR = 3.41, 95% CI [3.19, 3.65], *p* < .001, Cohen’s *d* = 0.68, 95% CI [0.64, 0.71].[[3]](#footnote-6)

*Are IA-AMP effects moderated by influence-awareness at the trial level?* We then extended the model by adding influence-awareness on each trial (influence-aware vs. non-influence-aware) as a fixed effect. This allowed us to determine if the relationship between Valence Rating and Prime Valence was moderated by that subset of influence-aware trials. Critically, an interaction between Prime Valence and influence-awareness emerged, such that IA-AMP effects were far stronger on that subset of trials on which participants reported being influenced by the prime, OR = 14.69, 95% CI [12.51, 17.26], *p <* .001, Cohen’s *d* = 1.48, 95% CI [1.39, 1.57].[[4]](#footnote-7)

*Are IA-AMP effects moderated by those participants who are more influence-aware?* We then sought to determine if IA-AMP effects were moderated by those participants who were more aware of prime influence in the IA-AMP (i.e., whether awareness rates varied between individuals and whether variation in this was associated with the magnitude of the IA-AMP effect). We therefore calculated an ‘awareness rate’ score for each participant by dividing the number of ‘aware’ trials by the total number of trials completed (i.e., 120). Note that the distribution of influence-awareness rates between participants is considered directly in the meta analysis section below. We then ran a linear regression analysis with IA-AMP effect size as the dependent variable and influence-awareness rate as a predictor variable. Results indicated that influence-awareness rate was a significant predictor of IA-AMP effect size, *B* = 0.41, 95% CI [0.31, 0.51], β= 0.54, 95% CI [0.41, 0.68], *p* < .001.

*Do online and post-hoc influence-awareness measures correlate with one another?* A simple correlation revealed that the online and post-hoc awareness measures strongly correlated with one another, *r* = 0.78, 95% CI [0.68, 0.88], *p* < .001.

*Does an online vs. post-hoc awareness measure predict IA-AMP effects?* Finally, we re-ran the regression analysis mentioned above while adding the two awareness measures to the model. This allowed us to determine the relative contribution of the online and post-hoc measures when predicting IA-AMP effects. Results indicated that both the online measure (*B* = 0.26, 95% CI [0.10, 0.42], β= 0.34, 95% CI [0.13, 0.55], *p* < .001) and post-hoc measure predicted IA-AMP effect sizes (*B* = 0.04, 95% CI [0.01, 0.06], β= 0.26, 95% CI [0.05, 0.47], *p* =.018). Comparison of the beta values and their confidence intervals indicated that the online measure was a better predictor of IA-AMP effects.

## Discussion

Results from Experiment 1 are in-line with our pre-registered hypotheses: namely that IA-AMP effects were driven by a specific subset of trials (i.e., those trials where participants reported been influenced by prime stimuli). Moreover, between participants, IA-AMP effects were also driven by those participants who were highly influence-aware. Finally, online and post-hoc measures of influence correlated strongly, and both measures uniquely predicted IA-AMP effects.

Taken together, our preliminary results verify the effectiveness of our ‘online’ measure of influence-awareness and highlight a key finding: IA-AMP effects are driven by a subset of influence-aware trials and by participants who are more highly influence-aware. Such a finding raises the question: do these subsets of trials and participants also drive performance in a traditional AMP? We sought to address this question in Experiment 2.

# Experiment 2: Influence-Awareness Rate on an IA-AMP Predicts AMP Effects on a Previously-Completed Traditional AMP

In Experiment 2, we intended to replicate and extend our initial findings in two ways. First, one might argue that an IA-AMP procedure does not merely assess the rate of awareness that people demonstrate during the AMP, but actually influences it, leading to the effects we obtained. Specifically, our modified task probed participants on a trial-by-trial basis as to whether they were influenced by the prime stimulus or not. This may have had a modulatory effect on the way in which they responded to trials within the IA-AMP. As such, it is difficult to know from Experiment 1 whether the predictive ability of influence rates can be generalized to a traditional AMP procedure. Thus our first aim was to assess the generalizability of our initial findings to traditional AMP effects. With this in mind, we required participants to first complete a traditional AMP followed by an IA-AMP from the same domain (i.e., evaluations of the Chinese characters in the context of generic positive and negative primes). In this way, we could initially index a traditional AMP effect for each participant, unperturbed by our IA-AMP manipulations, and then index an influence rate for each of those same participants in a separate procedure. If we were to find that influence-awareness rates in the latter task correlate with effect sizes in the former, then this would rule out the possibility that Experiment 1’s findings were simply due to something unique about the IA-AMP procedure. Rather, this would suggest that influence-awareness is central to effects produced in a traditional AMP.

Our second question related to a conceptual replication of Experiment 3 from Payne et al. (2013). Specifically, Payne and colleagues failed to find a significant difference between effects in a traditional AMP and those in the modified ‘skip’ AMP. On the basis of this, Payne and colleagues concluded that AMP effects are likely unaware, in the sense that providing an opportunity to skip unaware trials did not lead to any significant differences compared to when no ‘skip’ option was given. We sought to test this idea using our modified (IA-AMP) procedure. We wanted to know if a significant difference would emerge between the effects produced by a traditional AMP and an IA-AMP effect comprised only of ‘non-influence-aware’ trials. For our first question, we hypothesized that influence-awareness rates on an IA-AMP (completed second) would predict effect sizes on a traditional AMP (completed first). For our second question, we also expected that traditional AMP effects would be significantly larger than those generated on the basis of only the non-influence-aware trials on the IA-AMP. As such, our second hypothesis represented a conceptual replication of Payne et al. (2013) Experiment 3, but using greater power to detect differences where they previously observed none.

## Method

The pre-registration for this experiment is available at [osf.io/32cu7](https://osf.io/32cu7/).

**Participants.** Based on power analyses using identical criteria as Experiment 1, our a priori required sample size after exclusions was 150 participants. We used an identical sampling strategy to Experiment 1.[[5]](#footnote-8) 206 participants took part in the study and were paid £1.66. Of those, 176 (73 men, 102 women, 1 no gender given) ranging in age from 18 to 64 years (*M* = 33.60, *SD* = 11.45) provided complete data.

**Materials.** Two AMPs were employed in Experiment 2: a traditional AMP (consisting of 72 trials) with positive and negative primes and a similar IA-AMP to that outlined in Experiment 1, with the exception that the number of trials was reduced from 120 to 72. Results from Experiment 1 indicated that influence-awareness rates varied greatly between individuals. This between subject variation in the effect was of interest to us. Recognizing that participants were paid by the length of the experiment, in order to give greater opportunity to study this between subjects effect, we opted to direct our resources towards collecting relatively more participants with a smaller number of trials each.

**Procedure.** Participants first provided demographic information, followed by a traditional AMP, an IA-AMP, and the post-hoc awareness measure, in this order.

## Results

**Analytic Strategy**. In order to investigate the ability of influence-awareness rates to predict effect sizes in a previously completed traditional AMP, we utilized a linear regression model. Additionally, we used a paired-samples *t*-test to investigate differences between the traditional AMP effect size vs. uninfluenced-trials-only AMP effect size.

**Data Preparation**. Our data preparation was similar to that of Experiment 1 with two exceptions. First, we also computed an AMP score for each participant for the traditional AMP as well as the IA-AMP (and, like Experiment 1, were interested in the absolute magnitude of this effect). Second, we computed a ‘non-influenced-only’ AMP score for the IA-AMP. We calculated this score using the traditional AMP scoring method (see Experiment 1), but using only those trials in the IA-AMP on which participants did not press the spacebar (i.e., did not indicate awareness of influence of the prime in the response). As such, this score reflects the AMP effects generated on the basis of only uninfluenced trials.

**Replication Hypotheses.** *Do we find evidence for AMP effects and are those effects driven by influence-awareness?* A significant AMP effect emerged in both the traditional AMP (OR = 3.10, 95% CI [2.87, 3.35], *p* <.001, Cohen’s *d* = 0.62, 95% CI [0.58, 0.67]) and IA-AMP (OR = 4.66, 95% CI [4.30, 5.05], *p* <.001, Cohen’s *d* = 0.85, 95% CI [0.80, 0.89]). Consistent with Experiment 1, we found that IA-AMP effects were driven by that subset of trials where participants reported being influence-aware, OR = 20.65, 95% CI [17.10, 24.94], *p* <.001, Cohen’s *d* = 1.67, 95% CI [1.57, 1.77]. Also consistent with our first Experiment, we found that that effect sizes in the IA-AMP were predicted by the influence-awareness rates of participants, *B* = 0.44, 95% CI [0.34, 0.54], β= 0.56, 95% CI [0.44, 0.68], *p* < .001.

**Critical Hypotheses.** *Does influence-awareness on an IA-AMP completed at T2 predict*[[6]](#footnote-9) *people’s AMP effects at T1?* To answer this question we ran a regression analysis with the traditional AMP effect size as a dependent variable, and influence-awareness rate in the IA-AMP as a predictor variable. Results indicate that influence-awareness rate in the IA-AMP significantly predicted performance on the AMP that was completed at an earlier moment in time, *B* = 0.44, 95% CI [0.34, 0.54], β = 0.56, 95% CI [0.44, 0.68], *p* < .001.

*Does a non-aware-trials-only AMP effect different from a traditional AMP effect?* We wanted to know whether the AMP effect generated from only non-influence-aware trials in the IA-AMP differed from the AMP effect generated in the traditional AMP. Such an analysis is conceptually-similar to that employed by Payne et al. (2013) Experiment 3, who compared AMP effects in a traditional AMP to those in a modified AMP where participants could ‘skip’ trials where they felt they had been influenced. Whereas Payne et al. found no difference between their two conditions, we found that effects on the non-influence-aware-only trials in the IA-AMP (*M* = 0.24, *SD* = 0.29) were significantly smaller than for effects in the traditional AMP (*M* = 0.30, *SD* = 0.25), *t*(164.23) = 2.59, *p* = .010, Cohen’s *d* = 0.21, 95% CI [-0.01, 0.43].[[7]](#footnote-10)

## Discussion

We once again found that effects in the IA-AMP were driven by a subset of trials, and highly influence-aware participants. Perhaps more importantly, we found that influence-awareness rates in an IA-AMP completed at Time 2 predicted the magnitude of AMP effects in a traditional AMP at Time 1. Such a finding supports the notion that the findings obtained in Experiment 1 were not merely due to a contaminative effect of our modified task. Rather, the influence-awareness rate in the IA-AMP is relevant to the predicting effects on the traditional, unmodified AMP. Given that the AMP was completed before the IA-AMP, this strongly implies that the influence-awareness rate assessed within the IA-AMP is related to the (unassessed and unassessable) rate of influence-awareness in the traditional AMP.

Our results also conflict with prior work on the relative contribution of influence-aware trials in AMP effects. We found that AMP effects exclusively generated from non-influence-aware trials were significantly lower than AMP effects produced by a traditional AMP. This contrasts to previous work by Payne and colleagues (2013). One potential reason for this discrepancy is that our study was better powered than Payne et al.’s original work (i.e., *N* = 153 within-subjects design in this study versus a *N* = 72 between-subjects design in the original study). Additionally, recall that the original study committed an incorrect inference of equating absence of evidence of difference with evidence. We would argue, then, that our results represent a greater weight of evidence, and indicate that AMP effects are indeed stronger when participants are aware of the influence of the prime. Taken together, our findings thus far suggest that performance in the AMP is driven by a subset of trials, and highly influence-aware participants, and removing the subset of trials erodes the IA-AMP effect size, contrary to what has previously been claimed. This is considered in more detail in the meta-analyses section and is illustrated in Figure 2.

# Experiment 3: Influence-Awareness in an IA-AMP Predicts Performance on a Previously Completed Traditional AMP Even When the Two Assess Different Domains

Experiments 1 and 2 raise an entirely new set of questions. For instance, if AMP effects are driven by those participants who are highly influence-aware, and these participants are consistent across related AMPs, will these highly influence-aware participants remain consistent even when two AMPs assess *entirely unrelated* domains? That is, does the influence-awareness rate of a participant reflect a stable pattern of responding within the AMP that is consistent regardless of what domain is assessed (i.e., an individual differences trait)? Experiment 3 addressed this question by replicating Experiment 2 while changing the domains assessed by the standard (now using political prime stimuli: Barack Obama vs. Donald Trump) versus IA-AMP (using generic positively and negatively valenced prime stimuli, in the previous experiments). Specifically, we sampled participants who self-identified as US citizens and as supporters of the Democratic party, and exposed them to a traditional AMP with images of Donald Trump and Barack Obama as prime stimuli. The same participants then completed an IA-AMP containing the same positive and negative primes as used in Experiment 1 and 2. If influence-awareness rates reflect a stable (within-participant) pattern of responding regardless of content domain (politics vs generic positive/negative), then influence-awareness rates in the positive-negative IA-AMP should predict effect sizes within the standard political AMP.

## Method

The pre-registration for this experiment is available at [osf.io/uv3wk](https://osf.io/uv3wk/).

**Participants.** Based on power analyses using identical criteria to both of our previous experiments, our a priori required sample size after exclusions was 150 participants. We used an identical sampling strategy to our previous experiments. 175 participants took part in the study and were paid £1.66. Completed data from 155 (72 men, 81 women, 1 genderfluid, and 1 non-binary) ranging in age from 18 to 62 years (*M* = 31.74, *SD* = 10.18) was collected. All participants were US residents who identified politically as Democrats.

**Materials.** The IA-AMP was identical to that used in Experiment 2. The traditional AMP was also identical with one exception: primes now consisted of images of Donald Trump and Barack Obama rather than generic positive and negative images.

**Procedure.** The procedure was identical to that of Experiment 2.

## Results

**Analytic Strategy**. For this and all subsequent experiments, we divide results into two sections. First, ‘replicated hypotheses’ refer to hypotheses that were first made in one of our previous experiments and which are retested in the current experiment. These will be reported in brief format given that full explanations can be found in the relevant previous experiment. These results are also returned to in the meta analysis section below. Second, ‘critical hypotheses’ refer to new hypotheses made within this experiment. In all cases, we report only the key effects that serve to test our hypotheses. Other results of the models can be found in the Supplementary Materials on the Open Science Framework.

**Data Preparation**. Our data preparation strategy was similar to that of Experiment 2 with two exceptions: participants were required to be US residents and to identify politically as Democrats.

**Replicated Hypotheses.** *Do we find evidence for AMP effects and are those effects driven by influence-awareness?* We replicated our initial findings from Experiment 1-2. Specifically, a significant AMP effect emerged on the IA-AMP (OR = 3.42, 95% CI [3.15, 3.71], *p* <.001, Cohen’s *d* = 0.68, 95% CI [0.63, 0.72]), and traditional AMP (OR = 3.97, 95% CI [3.66, 4.32], *p* <.001, Cohen’s *d* = 0.76, 95% CI [0.72, 0.81]). IA-AMP effects were mainly driven by a subset of trials; namely, the influence-aware trials. IA-AMP effect sizes were once again predicted by influence-awareness rates (see Supplementary Materials).

**Critical Hypotheses.** *Does influence-awareness on a positive-negative IA-AMP completed at T2 predict people’s political AMP effects at T1?* A regression analysis with awareness rate in the IA-AMP as a predictor and AMP effect size in the traditional AMP as a dependent variable revealed that influence-awareness rates, as measured by the IA-AMP, significantly predicted traditional AMP effect sizes (*B* = .60, 95% CI [0.46, 0.74], β = .56, 95% CI [0.43, 0.69], *p* < .001).

## Discussion

Results indicate that influence-awareness in an IA-AMP retrospectively predicted the effect size of a traditional AMP, even when the two measures assessed attitudes towards different domains. We once again replicated our finding that AMP effects are driven by a subset of trials and highly influence-aware participants, and also showed that these highly influence-aware participants are consistent regardless of the domain assessed. It seems the AMP does not reflect the implicit evaluations of groups of people in general, but rather the evaluations of those individuals who are highly influence-aware.

# Experiment 4: The AMP’s Predictive Utility is Driven by Influence-Aware Trials

Experiments 1-3 indicated that AMP effects were driven by a subset of (influence-aware) trials and highly influence-aware participants, and that influence-awareness in an IA-AMP predicted performance in a traditional AMP completed prior. Remarkably, this effect was observed even when these two AMPs target evaluations towards different content domains. This suggests that the propensity for individuals to demonstrate AMP effects is a trait-like individual difference.

In Experiment 4 we extend our findings in three new ways. First, recall that in Experiments 2-3 we highlighted a unidirectional relationship between influence-awareness at Time 2 and AMP effects at Time 1. Now we wanted to determine if this relationship is actually bidirectional (i.e., if influence rates in AMP X predict effects in AMP Y, and if influence rates from AMP Y predict effects in AMP X). Determining this bidirectionality is important in order to demonstrate that *influence rates in general* are predictive of *AMP effects in general.* To investigate this we modified the design of Experiment 3 so that both AMPs were IA-AMPs, while holding the rest of the design constant.

Second, we examine if influence rates are consistent across AMPs. Experiments 2 and 3 provided indirect evidence that inter-individual influence-awareness rates are consistent and stable across AMPs by showing that influence-awareness rate on the IA-AMP predicted the absolute magnitude of the AMP effect on the traditional AMP (i.e., within-participant stability in influence-awareness rates was a prerequisite for this test). Experiment 4 tests this claim directly. Our working assumption thus far has been that the same participants drive effects in different AMPs *because* these participants have a consistent influence rate across AMPs (i.e., they are always highly influence-aware and this is why they demonstrate large effects). The direct demonstration of the stability of a participant’s influence rate is therefore important to substantiate our claim.

Finally, and perhaps most importantly, we wanted to know if the AMP’s predictive utility (in discriminating between two known-groups) was also dependent on influence-awareness. If AMP effects are driven by a subset of influence-aware trials and highly influence-aware participants, then those trials and participants should also contribute considerably to the measure’s ability to distinguishing between known-groups. If so, then this would suggest that the primary factor driving the measure’s predictive utility is the direct opposite of what has previously been thought (i.e., responding that is non-influence-aware). This would have serious implications for the interpretation of previous findings using the AMP to predict and assess group performances, as well as the rationale and utility of employing the AMP in the future.

## Method

The pre-registration for this experiment is available at [osf.io/mqp8v](https://osf.io/mqp8v/).

**Participants.** Initially, we conducted power analyses based on identical analyses to our previous experiments. Then we also assessed the suitability of the sample sizes derived from these power analyses for our novel analysis (i.e., separating Democrats and Republicans). Based on this, coupled with the availability of resources, our a priori required sample size after exclusions was 200 participants: 100 Democrats, and 100 Republicans. We used a highly similar sampling strategy to our previous experiments, with one exception: we firstly sampled from Democrats, and then from Republicans. 334 total participants took part and were paid £1.66. 207 participants (105 Democrats, 102 Republicans; 99 men, 106 women, 1 agender, and 1 no gender given) ranging in age from 18 to 65 years (*M* = 34.03, *SD* = 11.15) provided complete data.

**Materials.** Two IA-AMPs were employed. The first was a generic positive-negative IA-AMP that was identical to that used in Experiments 1-3. The second was also an IA-AMP, but employed the politics primes used in Experiment 3.

**Procedure.** Participants first provided demographic information, then completed a politics IA-AMP, a positive-negative IA-AMP, and a post-hoc awareness measure, in this order.

## Results

**Analytic Strategy**. Two linear regression models were used to assess the bidirectional ability of influence rates to predict AMP scores. A simple correlation test was used to determine the relative consistency of influence-awareness rates within participants on different AMPs. We also used two between-groups *t*-tests to investigate the relative predictive ability of influence-aware vs. non-influence-aware AMP scores in separating groups of individuals with different political beliefs.

**Data Preparation**. Our data preparation strategy was similar to that of Experiment 3 with three minor changes. First, participants were required to be US residents and to identify politically as *either* Democrats or Republicans. If they failed to do so they were excluded from subsequent analysis. Second, we computed three scores for the politics IA-AMP: an overall AMP score, an AMP score for uninfluenced trials only (as in Experiment 2), and an AMP score for influenced trials only (i.e., the same method for calculating the uninfluenced trials only score, but using only influenced trials instead). Third, where the directionality of AMP effects was important (i.e., where the differences between the AMP effects of Republicans and Democrats were of interest) we include the directionality (i.e., positive or negative values) of the AMP effect. In all other cases, we use the absolute values, as in our previous experiments.

**Replicated Hypotheses.** *Do we find evidence for AMP effects and are those effects driven by influence-awareness?* Significant AMP effects emerged for the positive-negative IA-AMP, OR = 3.27, 95% CI [3.05, 3.51], *p* < .001, Cohen’s *d* = 0.65, 95% CI [0.61, 0.69]. Given that we expected oppositional preferences for the primes between Republicans and Democrats, we assessed for political AMP effects by investigating the presence of a Prime Type \* Party interaction in predicting responses in the politics IA-AMP. We found the expected significant interaction effect, OR = 0.11, 95% CI [0.10, 0.13], *p* < .001, Cohen’s *d* = 0.87, 95% CI [0.82, 0.93]. Effects in both IA-AMPs were driven by that subset of trials where participants were influence-aware, and effect sizes for both IA-AMPs were predicted by the influence-awareness rates of participants in those IA-AMPs (see Supplementary Materials).

**Critical Hypotheses.**

*Does influence-awareness in one AMP predict performance in another AMP, and is this bidirectional?* We wanted to know if effects in one IA-AMP were predicted by influence-awareness rates in the other IA-AMP, and whether this relationship was bidirectional, regardless of the content of those IA-AMPs. To do so, we ran two regression analyses in which AMP effect sizes were to be predicted, and influence-awareness rate was the predictor variable. In the first regression, influence-awareness rate was taken from the politics IA-AMP, and the effect size from the positive-negative IA-AMP. In the second regression, influence-awareness rate was taken from the positive-negative IA-AMP and used to predict effect sizes in the politics IA-AMP. Results indicated that influence-awareness rates in the politics IA-AMP predicted scores in the positive-negative IA-AMP, *B* = 0.46, 95% CI [0.36, 0.56], β = 0.54, 95% CI [0.43, 0.66], *p* < .001, and that influence-awareness rates in the positive-negative IA-AMP predicted scores in the politics IA-AMP, *B* = 0.49, 95% CI [0.38, 0.60], β = 0.52, 95% CI [0.40, 0.63], *p* < .001.

*Are influence-awareness rates consistent within individuals on different AMPs?* We wanted to examine whether an individual’s influence-awareness rate was consistent between two IA-AMPs that assessed different content domains. That is, if influence-awareness rate represents a trait like individual difference. To do so, we correlated influence-awareness rates in the positive-negative and politics IA-AMPs. Results revealed a strong correlation between influence-awareness rates in the two tasks, *r* = 0.82, 95% CI [0.77, 0.86], *p* < .001.

*Does awareness drive the AMP’s predictive validity?* We investigated if the AMP’s ability to distinguish Democrats from Republicans was better on the basis of (a) non-influence-aware-only AMP scores or (b) influence-aware-only AMP scores. We conducted two *t*-tests between Democrats and Republicans: one comparing differences in aware-only AMP scores, and one comparing differences in non-influence-aware-only AMP scores. Our pre-registered hypothesis was that differences in AMP performance between the two groups would be larger when determined on the basis of influence-aware versus non-influence-aware trials. Consistent with this, we observed that IA-AMP effects calculated using the influence-aware trials were much better at discriminating between Democrats and Republicans (*d* =2.08, 95% CI [1.62, 2.55]) compared to IA-AMP effects calculated using the non-influence-aware trials (*d* = 0.62, 95% CI [0.33, 0.91]), *Q*(df = 1) = 27.51, *p* = .0000002.[[8]](#footnote-11) As shown in Figure 1, discriminability between the known-groups was primarily driven by those trials where people are aware of the influence of the prime on their responses.

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*Figure 1.* The politics IA-AMP’s ability to discriminate between Democrats and Republicans on the basis of influence-aware and non-influence-aware trials. Error bars represent 95% confidence intervals.

## Discussion

Results indicate that AMP effects generated from aware-only trials were far superior in discriminating between two known-groups (Democrats vs. Republicans) than those generated from non-influence-aware trials. Additionally, the impact of influence-awareness rates on the AMP’s predictive utility was also found to be bidirectional (i.e., influence-awareness rates in the politics AMP predict positive/negative AMP score and vice-versa). Moreover, we found that influence rates between AMPs were strongly correlated despite the fact that the two AMPs were targeting entirely different content domains. These findings provide yet further support for the idea that (a) AMP effects are driven by a subset of influence-aware trials and those highly influence-aware participants, and (b) that the influence-aware participants who drive AMP effects in one domain are the same participants who drive effects in another domain. Critically, they also imply that the AMP’s predictive validity is heavily driven by the trials, which are influence-aware. While the non-influence-aware trials retain some degree of predictive validity, results suggest that when it comes to responses on the AMP, that which is useful is not particularly implicit, and that which is implicit is not particularly useful.

# Experiment 5: Recent Modifications to the AMP do not Reduce the Impact of Influence-Awareness on AMP Effects

We are not the first to argue that AMP effects are driven by highly influence-aware participants. In a recent review of the AMP literature, Mann et al. (2019) noted that data from AMP studies exhibit a strong bimodal distribution, with a subset of participants showing a very strong AMP effect, and another producing scores that follow a normal distribution (also see Bar-Anan & Nosek, 2012). Mann et al. argued that this cluster of extreme scoring participants (i.e., those who produce the bimodality) represent a small group of intentional responders, whereas the remaining participants in the normal distribution reflect unintentional responders.

Mann et al. attempted to eliminate the contaminating influence of these intentional responders (and thus reduce this bimodality) by creating a new and improved variant of the AMP. This variant employed visually stimulating paintings as target stimuli, rather than the less visually stimulating Chinese characters, in order to increase attention to the target rather than prime. They also added more detailed instructions that strongly implored participants to avoid intentional responding to the prime while reassuring them that it was acceptable if they sometimes did so. The authors concluded that their modifications to the AMP decreased bimodality compared to a traditional AMP.

Experiments 1-4 also consistently found that group-level AMP effects are driven by highly influence-aware participants. Mann et al.’s modified AMP has been argued to effectively eliminate similar subset effects and might therefore help overcome the various issues we consistently found in our studies. Therefore, Experiment 5 sought to test if the impact of influence-awareness persists or is eliminated (at both the trial and individual levels) when the Mann et al. AMP is used. We conducted an exact replication of the first two hypotheses from Experiment 2 while replacing the traditional AMP with the Mann et al. AMP (referred to herein as the ‘Mann AMP’), and the IA-AMP with a Mann et al. IA-AMP (referred to herein as the ‘Mann IA-AMP’). We chose to implement our design from Experiment 2 because the effects noted in Experiments 3 and 4 are nested within the underlying issue of influence-awareness predicting AMP effects which is demonstrable in Experiment 2. If the Mann AMP did not show the same effects as the traditional AMP did in Experiment 2, then it would also not show those effects conditional on this in Experiments 3 and 4. Likewise, if the Mann AMP did show the same effects as the traditional AMP in Experiment 2, then it is subject to the same issues that give rise to the effects seen in Experiments 3 and 4.

We pre-registered two hypotheses, similar to Experiment 2. First, we hypothesized that, at both the trial-level and the participant-level, effects in the Mann IA-AMP would still be driven by influence-aware trials. Second, we hypothesized that the influence-awareness rates of a given participant in the Mann IA-AMP would predict the effect of that same person in a previously completed Mann AMP. We were unsure whether or not Mann et al.’s modifications would reduce the role of influence-awareness in producing AMP effects. However, because we were attempting to replicate the effects of Experiment 2, we approached our hypotheses similarly to Experiment 2 (i.e., that influence-awareness would still predict AMP effects). That is our question was whether the Mann et al. modifications would remove, not merely reduce, the problematic associations between AMP effects and influence-awareness.

## Method

The pre-registration for this experiment is available at [osf.io/35b6p](https://osf.io/35b6p/).

**Participants.** To power these analyses, we examined the association between the IA-AMP influence-awareness rate and the absolute AMP effect observed in Experiment 2, to which this experiment was most similar. Results from Experiment 2 indicated this association to be in the range β = 0.56, 95% CI [0.44, 0.68]. However, on the basis that we were unsure whether the Mann et al. alterations would impact the magnitude of this association compared to our previous studies, we opted to power our analyses to detect a smaller effect size (i.e., β = .20). We determined that in order to power a regression analysis to detect a β = .20 at a 0.05 alpha level (two-tailed) with 95% power, we would require 320 participants. We defined this as our a priori sample size after exclusions. We again used an identical sampling strategy to our previous experiments. 410 participants took part in the study and were paid £0.95. Of those, 330 (158 men, 171 women, 1 agender) ranging in age from 18 to 65 (*M* = 33.40, *SD* = 11.05) provided complete data.

**Materials.** We employed two AMPs in this experiment: Mann et al.’s version of the traditional AMP, and a Mann et al. version of the IA-AMP (i.e., a Mann AMP with the option to press the spacebar if aware of influence by the prime stimulus). In line with Mann et al. (2019), each AMP consisted of 60 trials. All other parameters of these AMPs were identical to the AMP of Mann et al., with one exception: rather than use face images as prime stimuli, we used positively- and negatively-valenced images (identical to those used in Experiment 2).

**Procedure.** The procedure was identical to Experiment 2 with the exception that the traditional AMP was replaced with Mann et al.’s AMP, and the IA-AMP with a Mann et al. variant of our IA-AMP.

## Results

**Analytic Strategy**. Our analytic strategy was identical to that of Experiment 2.

**Data Preparation**. Our data preparation was identical to that of Experiment 2.

**Replication Hypotheses.** *Do we find evidence for Mann (IA-)AMP effects?* As with Experiments 1-4, we first sought to verify the presence of AMP effects in both the Mann AMP and the Mann IA-AMP using logistic mixed-effects models. We observed a significant effect on both the Mann AMP, OR = 3.72, 95% CI [3.48, 3.98], *p* < .001, Cohen’s *d* = 0.72, 95% CI [0.69, 0.76], and the Mann IA-AMP, OR = 4.36, 95% CI [4.08, 4.67], *p* < .001, Cohen’s *d* = 0.81, 95% CI [0.77, 0.85].

**Critical Hypotheses.** *Does influence-awareness predict Mann IA-AMP effects at the trial level and individual level?* At the trial-level, we used a linear mixed-effects model similar to that of our previous experiments (i.e., Prime Type and influence-awareness as IVs, Response as DV) in order to determine the role of influence-awareness in producing effects in the Mann IA-AMP. The hypothesized interaction between influence-awareness and Prime Type was significant, OR = 16.30, 95% CI [13.79, 19.28], *p* < .001, Cohen’s *d* = 1.54, 95% CI [1.45, 1.63]. At the participant-level, we conducted a similar linear regression analysis to that used in our previous experiments (i.e., Influence Rate as IV, and Mann IA-AMP Effect as DV). We found that, as in our other experiments, Influence Rate significantly predicted Mann IA-AMP Effect, *B* = 0.54, 95% CI [0.47, 0.62], β = 0.61, 95% CI [0.53, 0.70], *p* < .001.

*Does influence-awareness on a Mann IA-AMP completed predict effects on a previously Mann AMP effects?* In order to investigate the extent to which influence-awareness rates in the Mann IA-AMP could predict effects in a previously-completed Mann AMP, we used an identical model to the linear regression detailed above, but substituted the DV from the IA-AMP effect to AMP effect. With this model, we found that Influence Rates in the IA-AMP predicted scores in the previously-completed AMP, *B* = .38, 95% CI [0.30, 0.47], β = .42, 95% CI [0.32, 0.52], *p* < .001.

**Non Pre-Registered Analyses.** *Does the predictive utility of influence-awareness vary between the traditional AMP and Mann AMP?* Following data collection, we noted that effect sizes in the latter analysis (i.e., Influence Rate predicting AMP effects) appeared relatively similar to our analysis in Experiment 2. While our preregistered hypothesis considered the presence of such an association, in retrospect it also seemed useful to evaluate whether this effect was weakened by the Mann et al. modifications. That is, even though the Mann et al. modifications still showed a non-zero effect of influence-awareness, it may have been the case that the modifications reduced this effect relative to those seen in the traditional AMP in Experiment 2 (and our previous analyses did not examine this relative difference). We therefore conducted additional analyses to determine if effect sizes for this analysis differed significantly across the two experiments. We pooled the data from Experiments 2 and 5 and then constructed a similar regression model as used in those experiments (i.e., Influence Rate as IV, [Mann] AMP effect as DV), also adding AMP type (i.e., Experiment) as a fixed effect in the model. If Influence Rate significantly differed in how well it predicted AMP effects between the traditional AMP and Mann AMP, then we would expect an interaction between Influence Rate and AMP type (i.e., Experiment). Analyses revealed no such interaction between Experiment and Influence Rate (*B* = 0.04, 95% CI [-0.09, 0.18], β = 0.04, 95% CI [-0.10, 0.19], *p* = .534). In order to quantify evidence for the absence of this interaction, we computed a Bayes Factor for this interaction effect using the BayesFactor R package (Morey & Rouder, 2019) by comparing models within and without this interaction effect. This Bayesian analysis using the default prior (Cauchy distribution placed on the effect size with scaling factor *r* = 0.5) revealed moderate evidence in support of the null hypothesis, BF10 = 0.12.

## Discussion

In line with our pre-registered hypotheses, the use of a modified AMP procedure designed to eliminate a subset of intentional responders (i.e., the Mann et al. AMP) did not reduce the impact of influence-awareness in predicting AMP effects, either at the trial- or participant-level. Within the Mann IA-AMP, effects were also driven by a subset of influence-aware trials (within participants), and highly influence-awareness participants (between participants). Influence-awareness rates in the Mann IA-AMP also predicted effects sizes in a previously completed Mann AMP. The prediction of AMP effect sizes by influence rate in the Mann AMP did not significantly differ from, and was credibly equivalent to, what was seen in Experiment 2 using a traditional AMP. In short, we obtained the same pattern of outcomes as reported in Experiments 1-4 with a variant of the AMP specifically designed to eliminate subset effects seen in other AMP research.

# Meta-analyses

We decided to meta-analyze our data for multiple reasons. First, it allowed us to estimate our effect sizes with much greater precision. Second, given that methodologies varied between the experiments (e.g., sample sizes, number of trials in the IA-AMP, the domain being assessed, etc.), the use of random-effect models allowed us to use any observed heterogeneity among our experiments to estimate the likely range of effect sizes to be observed in future studies employing varying methodological alterations (i.e., the credibility interval). Meta analyses were conducted using the metafor R package (Viechtbauer, 2010) using a Restricted Maximum Likelihood meta analysis models. Multilevel meta analytic models were employed in order to (a) acknowledge non-independence of the multiple IA-AMPs completed by participants in Experiment 4, and (b) in order to acknowledge the differences in the domain being assessed between AMPs/IA-AMPs (i.e., politics vs. generic positive/negative primes). Meta-analyses were not pre-registered, although the hypotheses assessed within them are identical to those pre-registered in the original experiments.

*Does influence-awareness predict IA-AMP effects at the trial level?* We first meta-analyzed the hypothesis that, at the trial level, the impact of the primes’ valence on evaluative ratings in the IA-AMPs is moderated by the subset of influence-aware trials. Experiment was entered as a random factor in the model. As in the component experiments, analyses revealed a significant meta-analytic interaction effect between Prime Type and influence-awareness in predicting IA-AMP effects, such that larger effects were observed on the influenced trials, OR = 21.36, 95% CI [15.85, 28.79], 95% CR [10.54, 43.30], *p* < .001 (*p* < 10-89), Cohen’s *d* = 1.69, 95% CI [1.52, 1.85], 95% CR [1.30, 2.08]. This moderation of the IA-AMP effect by influence rate is illustrated in Figure 2. In order to better understand the nature of this interaction effect, we also calculated a meta effect size for the main effect for Prime Type (which is comprised of both influenced and uninfluenced trials), OR = 3.78, 95% CI [3.28, 4.36], *p* < .001, Cohen’s *d* = 0.73, 95% CI [0.65, 0.81]. A comparison of the main (OR = 3.78, *d* = 0.73) and interaction effects (OR = 21.36, *d* = 1.69) demonstrates that the interaction was substantially and significantly larger than the main effect. What this shows is that influence-awareness on a given trial does not merely augment an otherwise strong IA-AMP effect. Rather, influence-awareness represents theprimary driving force of the IA-AMP effect.

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*Figure 2.* Across experiments, IA-AMP effects are strongly moderated by the proportion of influence-aware trials of the participant.

*Does influence-awareness predict IA-AMP effects at the participant-level?* Second, we meta-analyzed the hypothesis that, at the participant level, the absolute magnitude of the IA-AMP effect is moderated by those participants who are highly influence-aware. Experiment and domain were entered as nested random effects in the model. As in the component experiments, results demonstrated that the influence-awareness rate in the IA-AMP predicted the absolute IA-AMP effect sizes, *B* = 0.52, 95% CI [0.45, 0.58], 95% CR [0.37, 0.66], *p* < .001 (*p* < 10-54). Again, in order to better understand the nature of this effect, we also meta analyzed the intercept (i.e., the IA-AMP effect across both high and low influence-aware participants), *B* = 0.31, 95% CI [0.29, 0.33], *p* < .001. Results demonstrated that the main effect for influence-awareness rate (*B* = 0.52) was substantially and significantly larger than the intercept (*B* = 0.31). What this shows is that the influence-awareness rate does not merely augment an otherwise strong group-level IA-AMP effect. Rather, influence-awareness represents theprimary driving force of IA-AMP effects, both within and between participants.

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*Figure 3.* Influence-awareness rates on the IA-AMP predict the magnitude of the previously completed AMP, even when the IA-AMP and AMP assess different domains.

*Does influence-awareness in the IA-AMP predict effect sizes in a previously completed AMP?* Third, we meta analyzed the hypothesis that the influence-awareness rate assessed by the IA-AMP is predictive of the traditional AMP effect, even though (a) the AMP was always completed prior to the IA-AMP, and (b) in some cases, the AMP measured a completely different domain to the IA-AMP. This would provide strong evidence that (a) this effect is general to the AMP rather than being specific to the IA-AMP, (b) cannot merely be the product of bringing influence-awareness to the attention of the participant, and (c) represents a general effect across AMPs regardless of the domain each assesses. Participant level data was taken from Experiments 2, 3 and 5, each of which included both an AMP and an IA-AMP. As in the component studies, results demonstrated that the influence-awareness rate in the IA-AMP predicted the absolute effect size in the previously completed AMP, *B* = 0.43, 95% CI [0.29, 0.58], 95% CR [0.16, 0.71], *p* < .001 (*p* < 10-8). This effect, split by domain congruence between the AMPs and IA-AMPs, is illustrated in Figure 3. In order to better understand the nature of this effect, we also meta-analyzed the intercept (i.e., the AMP effect across both high and low influence-aware participants), *B* = 0.32, 95% CI [0.27, 0.36], *p* < .001. Results demonstrated that the main effect for influence-awareness rate (*B* = 0.43) was substantially larger than the intercept (*B* = 0.32). Once again, this shows is that influence-awareness rate does not merely augment an otherwise strong group-level AMP effect. Rather, influence-awareness represents theprimary driving force of AMP effects between participants.

*What is the distribution of influence-awareness across participants?* Fourth, in order to understand how prevalent influence-awareness was across participants, data was pooled from Experiments 1-5 (total *N* = 1021). Visual inspection of distribution kernel-density plots and histograms using multiple smoothing and binning parameters demonstrated clear bimodality (see Figure 4). As such, we opted to summarize influence-awareness rates using robust metrics rather than to meta-analyze them. The median influence-awareness rate of participants was 0.17 (median absolute deviation = 0.25). Calculating quintiles demonstrated that 54% of participants were influence-aware on 0-20% of trials, 14% were influence-aware on 21-40% of trials, 8% were influence-aware on 41-60% of trials, 6% were influence-aware on 61-80% of trials, and 17% were influence-aware on 81-100% of trials. Quantiles were also used to illustrate the extremity of scores, with 33% of participants demonstrating almost no influence-awareness (≤ 5%) and 9% demonstrating almost full influence-awareness (≥ 95%).

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*Figure 4.* Bimodality in the distribution of participants’ influence-awareness rates in the IA-AMPs pooled across Experiments 1-5.

# Structural Validity:

# ‘Implicitness’ aside, does the (IA)AMP even represent a valid measure of evaluations?

The previous analyses focus on the question of whether the AMP effect can be considered as an implicit measure. Results demonstrate that the AMP effect is driven by influence aware responses (within participants) and the participants who are more highly influence aware (between participants). The AMP is therefore not implicit in the sense of unaware.

One point we highlight later in the General Discussion section is that researchers may be tempted to view the IA-AMP’s influence-aware trials as contamination or noise, and to create an AMP variant and exclusively use responses from the non-influence aware trials of that task to create a better implicit measure. On the one hand, and as we caution in the General Discussion, data from our Influence-Awareness AMP’s *non-influence aware* trials should not necessarily be interpreted as representing *influence unaware* responding. For this, a different task would have to be developed and tested (e.g., an Influence-Unawareness AMP). On the other hand, it is perhaps useful to explore the utility of the non-influence aware trials as a (possibly implicit) measure of evaluations, while bearing the above caveat in mind.

For the sake of these analyses, let us take the best-case scenario and assume that non-influence aware responses somehow do represent influence unaware responses. This would certainly mean that the effect itself would qualify as ‘implicit’ in the sense of unaware. Yet the question of whether responding on these trials represent a *measure* of evaluations still remains. In other words, is the IA-AMP effect a structurally sound *measure of evaluations* at all?

The current analyses examine this latter question by examining the AMP’s structural validity. Recent work indicates that assessments of a measure’s structural validity are vital to theoretical development reliant on the results of that measure. Yet thorough consideration of structural validity is often overlooked (Flake & Fried, 2019; Hussey & Hughes, 2018). We therefore examine the AMP’s structural validity.

Throughout our manuscript we employed a common method of scoring the AMP: the difference score between evaluations of trials preceded by one prime type (e.g., positive stimuli) versus trials preceded by the other prime type (e.g., negative stimuli)[[9]](#footnote-12). Such an approach means that a specific type of measurement model and factor structure is consistently assumed to generate AMP effects - namely - two *negatively correlated* latent variables (for a discussion of the latent variable structures implied by simple scoring metrics see Rose, Wagner, Mayer, & Nagengast, 2019). For example, on the positive-negative IA-AMP, ratings on trials preceded by positive primes should be negatively correlated with ratings of trials preceded by negative primes (regardless of whether the influence of primes occurs inside or outside of awareness). The corollary of this point is that, were positive correlations between these trial types to be found, this would imply that the primes are not influencing target evaluations, and therefore the task is not functioning as a sound measure of target evaluations as a function of these primes. With this in mind, we assessed the directionality of the correlations between ratings between the two prime types as a simple test of the AMP’s structural validity.[[10]](#footnote-13) Comparable to previous analyses, we also examined whether this correlation was moderated by influence-awareness, both within and between participants.

## Structural validity of the IA-AMP between influence-aware and non-influence aware trials

We first assessed the IA-AMP’s structural validity within participants (i.e., between influence-aware and non-influence aware trials). IA-AMP data was taken from Experiments 1-5. Mean ratings for each prime type were calculated for each participant, separated by influence aware and non-influence aware trials. A linear mixed-effects model was constructed using the lme4 R package (Bates, Mächler, Bolker, & Walker, 2015), with mean ratings of Prime Type B trials (e.g., negative images) as the DV, and mean ratings of Prime Type A trials (e.g., positive images) and Influence Awareness as the IVs. Experiment and AMP domain were entered as crossed random effects. The interaction effect represented the key hypothesis test. Results demonstrated that the correlation between ratings of the Prime Type A and Prime Type B trials on the traditional AMP was strongly moderated by participants’ influence-awareness rate in the later IA-AMP, *B* = 0.63, 95% CI [0.55, 0.71], β = 0.75, 95% CI [0.65, 0.85], *p* < .0001 (*p* < 10-50). Specifically, participants’ ratings on the two prime type trials were negatively correlated only on influence aware trials. This demonstrates that the primes only exert influence on ratings within the IA-AMP when participants are influence-aware (see Figure 5). As such, while it could be argued that non-influence aware trials on the IA-AMP represent ‘implicit’ responding, these trials do not function as a structurally valid measure of evaluations.

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*Figure 5.* Correlations between ratings on the IA-AMP between the two prime types are negative only when influence-awareness is high. Positive correlations imply violations of structural validity.

## Structural validity of the AMP across participants’ influence-awareness rates

We also assessed the traditional AMP’s structural validity between participants who demonstrated different levels of influence-awareness. As in the third meta-analysis in the main manuscript, suitable data were taken from experiments that included both an AMP and IA-AMP (i.e., Experiments 2, 3, and 5). Mean ratings for each prime type on the AMP were calculated for each participant, as well as their influence-awareness awareness rate on the IA-AMP. A linear mixed-effects model was constructed with mean ratings on Prime Type B trials (e.g., negative images) as the DV, and mean ratings of Prime Type A trials (e.g., positive images) and influence rate as the IVs. Experiment and AMP domain were entered as crossed random effects. The interaction effect represented the key hypothesis test. Results demonstrated that the correlation between ratings of the Prime Type A and Prime Type B trials on the traditional AMP was strongly moderated by participants’ influence-awareness rate in the later IA-AMP, *B* = -0.58, 95% CI [-0.79, -0.37], β = -0.41, 95% CI [-0.55, -0.26], *p* < .0001 (*p* < 10-7).

Description: Macintosh HD:Users:Ian:git:amp-intentionality:structural validity:p6.pdf

*Figure 6.* Correlations between ratings on the traditional AMP between the two prime types are negative only when participants’ influence-awareness rate on the IA-AMP is high (> 66%). Positive correlations imply violations of structural validity.

Specifically, participants’ ratings on the two prime type trials were negatively correlated (as required) when their influence-awareness rates were high. However, positive correlations were found when influence-awareness rates were low. This strongly suggests that the primes only exert influence on ratings within the AMP task when participants are highly influence-aware (see Figure 6). Specifically, the tipping point at which correlations become negative and the measure begins to demonstrate this aspect of structural validity was an influence-awareness rates of > 66%. As such, results show that the AMP demonstrates violated this aspect of structural validity in 79% of participants.

## Interim Conclusion

Although non-influence aware trials could perhaps be argued to be ‘implicit’ (on the assumption that non-influence awareness reflects unawareness), these analyses suggest that such trials do not demonstrate a key aspect of structurally validity. As such, although these trials could perhaps be argued to have the status of being ‘implicit’, these trials lose the more important quality of being a *measure of evaluations*. At the participant level, although low-influence aware participants could perhaps be argued to be providing more ‘implicit’ responses (conditional on the same assumption as above), results suggest that the AMP cannot be said to represent a structurally sound *measure of evaluations* in these participants. Given the distribution of influence-awareness rates, the AMP does not provide a structurally sound measure of evaluations in 79% of participants. Together, these suggest that non-influence aware responding on the IA-AMP cannot be said to represent a structurally sound implicit measure of evaluations.

The tradeoff between implicit and non-implicit responding therefore appears to go beyond implications for statistical power (see General Discussion), and also has a bearing on the task’s structural validity – the degree to which scores can be said to represent a sound *measure of evaluations* at all. For the majority of participants, this seems to not be the case.

# General Discussion

The vast majority of AMP research has assumed that effects on the task are driven by the *misattribution* of a prime’s valence to a target stimulus without one’s *intention* or *awareness* that this is happening (Payne et al., 2005)*.* AMP effects are therefore said to be ‘implicit’ to the extent that the mental process mediating a participant’s task performance (misattribution) operates under two automaticity conditions (unintentional and unaware). Although several papers seem to support this idea (Gawronski & Ye, 2014; Payne et al., 2013), they are each subject to methodological, statistical, and conceptual issues that limit their evidential value. Across five pre-registered and highly powered studies, we sought to overcome those issues, test the above assumptions, and offer a clearer insight into the ‘implicitness’ of the AMP effect. Our goal was to determine if people are in fact aware of the prime’s influence on their evaluations, and if it is this awareness that drives AMP effects.

## Overview of Findings

Each of our studies employed at least one IA-AMP that was designed to capture influence-awareness in a trial-by-trial, ‘online’ fashion. Across experiments, IA-AMP effects were consistently driven by the subset of trials in which people reported awareness of the prime’s influence on their evaluations. This is at odds with a core premise underpinning the AMP’s use. Moreover, influence-awareness was found to drive not only the magnitude of the IA-AMP effect but also its predictive validity: Experiment 4 showed that IA-AMP effects calculated using only influence-aware trials discriminated between participants’ political orientations (*d* = 2.08, 95% CI [1.62, 2.55]) far better than effects computed using only non-influence-aware trials (*d* = 0.62, 95% CI [0.33, 0.91]).

At the participant level, influence-awareness rates (i.e., the proportion of trials a person reports as being influenced-aware) varied substantially and bimodally between individuals, with the majority of people demonstrating a low rate (more than half of participants < 20%) and a small subset of people demonstrating high overall influence (17% of participants > 80% influence rate; see Figure 4). Critically, this influence-awareness rate predicted the absolute magnitude of IA-AMP effects between participants (Experiments 1-5: *B* = 0.52, 95% CI [0.45, 0.58] in meta analysis). In short, a subset of influence-aware trials, and participants who were highly influence-aware, drove IA-AMP effects and their predictive validity.

When reflecting on these findings, one may be tempted to ask whether there something special about our the IA-AMP and therefore, more importantly, whether our results are applicable to the traditional AMP. For example, perhaps by prompting people on every trial to reflect on the influence of the prime on their evaluations we inflated the very influence-awareness we were attempting to measure. This is highly unlikely given that influence-awareness rates on the IA-AMP were strongly predictive of not only the IA-AMP effect itself, but also the magnitude of effects on a previously completed traditional AMP (*B* = 0.43, 95% CI [0.29, 0.58] in meta analysis). Because the traditional AMP was always completed prior to the IA-AMP, effects on the traditional AMP were unperturbed by the modifications within the IA-AMP. This result held both when the traditional AMP and IA-AMP assessed the same domain (Experiments 2 & 5), but, even more interestingly, also when they assessed different domains (e.g., political primes vs. simple valenced primes: Experiments 3 & 4). The strongest demonstration of this can be found in Experiment 3, where the influence-awareness rate on an IA-AMP assessing one domain (generic positive/negative primes) predicted a previously completed traditional AMP effect in a different domain (political primes). Taken together, we found strong evidence that traditional AMP effects are driven by highly influence-aware people and a subset of influence-aware trials.

Perhaps most interestingly, the participants who drive the AMP effect were consistent *across* AMPs. This was illustrated by the fact that (a) influence-aware rates predicted the magnitude of effects in previously-completed AMPs (Experiments 2-5, as discussed above), and (b) influence-aware rates correlated very strongly between two IA-AMPs in different domains (Experiment 4: *r* = 0.82, 95% CI [0.77, 0.86]). This implies that the propensity to demonstrate an effect on the AMP *in general*, regardless of the domain under investigation or the direction of the effect, may be a state- or trait-like individual difference (the implications of this finding are discussed below).

Other researchers have previously noted that a subset of participants exert a disproportionate influence on AMP effects: Mann et al. (2019) recently argued that a subset of participants tend to intentionally evaluate prime rather than target stimuli, and thus contaminate the measure. They devised a modified AMP that purportedly eliminated this confound. In Experiment 5, we examined if applying the Mann et al. modifications to the AMP and IA-AMP would remove this effect. Results indicated that Mann et al.’s manipulations did not reduce this effect relative to the traditional AMP.

To conclude, our findings demonstrate that (a) the AMP effect and its predictive validity are primarily driven by (non-implicit) influence-aware responding, (b) influence-awareness rates vary widely between individuals but are highly consistent within individuals, even across domains, (c) participants who are more highly influence-aware drive AMP effects, (d) recent modifications to the AMP that purportedly control for such subsample effects do not reduce or resolve this issue, and (e) its implicitness aside, the AMP effect was shown to lack a key aspect of structural validity in the majority of participants (i.e., the 79% of participants for whom influence-awareness rates were < 66%).

Table 1. *Premises of past and present research on the AMP and their logical conclusions*

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | Research question/issue | | | |
|  | Implicitness | | Mechanism | Validity |
|  | Awareness | Intentionality |  |  |
| 1. Common theoretical premises | AMP effects are implicit in the sense of unaware | AMP effects are implicit in the sense of unintentional | AMP effects are mediated by misattribution | The AMP effect is a valid measure of evaluations |
|  | - | - | Misattribution only occurs in the absence of awareness | - |
| 2. Premises based on previous work | Previous literature on the AMP effect’s unawareness contains many statistical, methodological, and conceptual issues that weaken its conclusions | Previous literature on the AMP effect’s unintentionality contains many statistical, methodological, and conceptual issues that weaken its conclusions | - | - |
| 3. Premises derived from the current work | Our results suggest that AMP effects are driven by awareness | - | Our results suggest that AMP effects are driven by awareness | AMP effects demonstrate structural invalidity in the majority of participants (i.e., when influence-awareness is not high) |
| Logical conclusions | The AMP effect is not implicit in the sense of unaware | There is no clear evidence to suggest that the AMP is implicit in the sense of unintentional | The AMP effect is not mediated by misattribution | The AMP effect is not a structurally-valid measure of evaluations for the majority of individuals |
| *Notes:* Premises and conclusions are arranged vertically in columns, so that conclusions follow from the premises above them. | | | | |

Table 1 provides a wealth of information regarding the premises underpinning AMP research and our novel conclusions. Relevant citations for these premises are provided in the Supplementary Materials. The table’s conclusions (1) suggest the AMP effect is not implicit (in the sense of unaware), (2) question misattribution (as traditionally-conceived) as the AMP effect’s mediating mechanism, and (3) call into question whether the AMP effect is a valid measure of evaluations across individuals.

It is worth noting that we also arrive at a fourth conclusion. Due to methodological, statistical, and conceptual issues present in past work, we currently lack strong evidence that the AMP effect is implicit in the sense of unintentional (see Introduction). We believe it is incumbent on those looking to support that claim to provide strong evidence for it (rather than for those who disagree with it to prove a negative).

Despite the evidentiary weight of the first three conclusions, many readers may still be tempted to focus on the fourth, one which only represents a single cell of the table - one which is tangential to what we actually set out to test. However tempting this may be, we would encourage the reader to also focus on all the other cells of the table given that these constitute what the paper actually set out to test and what we actually found. We do so for good reason: the question of unintentionality should not occupy center stage when so many other premises of the AMP fail to hold.

## Implications

**Conceptual implications: Is the AMP an implicit measure?** The outcome of a measurement procedure may be considered implicit if it captures a cognitive process under one or more conditions of automaticity*.* AMP effects have historically been argued to be implicit in the sense that they occur without awareness or intention (Payne et al., 2005). Yet our findings consistently show that AMP effects are driven by awareness, contrary to what is commonly claimed. When combined with the methodological, statistical, and conceptual issues present in past work (see introduction), compelling evidence for the implicitness of the AMP is currently lacking. Taken together, the trend of evidence does not support claims made by the *implicit misattribution account* of the AMP*.*

Of course, it may be that AMP effects are still ‘implicit’ in the sense that they are emitted with awareness but under other automaticity conditions. Take, for instance, unintentionality: it may be that participants are very much aware of the primes’ influence on their target evaluations, but are nonetheless unable to help themselves from responding in a manner consistent with the prime’s valence. However, as we have already argued, the literature on the (un)intentionality of the AMP has a number of statistical, methodological, and conceptual issues which arguably undermine the credibility of their conclusions. In this sense, strongly compelling evidence for the unintentionality of AMP effects is currently lacking. Additionally, there has been little investigation into whether AMP effects possess other automaticity features (e.g., fast or efficient). This is particularly problematic given that a vast array of conceptual, theoretical, and empirical claims have been made based on the idea that AMP effect are implicit: for instance, claims about the automaticity of processes underlying evaluative conditioning (Jones, Fazio, & Olson, 2009), anorexia nervosa (Spring & Bulik, 2014), biases underlying racially-biased policing (Spencer, Charbonneau, & Glaser, 2016), and affective responses which predict dysfunctional drinking (Payne, Govorun, & Arbuckle, 2008). Despite nearly 15 years of research, methodologically strong and replicable evidence for the AMP effect’s implicitness is still worryingly absent.

**Theoretical implications: Do AMP effects reflect a misattribution process?** As we mentioned in the introduction, most researchers subscribe to the idea that AMP effects are mediated by the misattribution of prime valence to the target stimulus (Bar-Anan & Nosek, 2012; Gawronski & Ye, 2015; Payne et al., 2005; Payne et al., 2013). Misattribution is traditionally conceived of as occurring in the absence of awareness (Schwarz & Clore, 1983) and, as Payne and colleagues (2005) argued, can only occur when people are unable to control their responses. The fact that AMP effects rely heavily on awareness of prime influence suggests two possibilities. On the one hand, AMP effects may be driven by misattribution, as is often claimed, yet people are fully aware that misattribution is taking place. This would allow for the concept to be retained in some modified form. However, such an approach runs contrary to how misattribution is traditionally defined (Schwarz & Clore, 1983), and would require an overhauling of the concept itself. Yet even if a redefinition of the construct were undertaken, our findings suggest that misattribution would still be occurring or captured in only those participants who were highly influence-aware, rather than people *in general* (we return to this idea later on). As such, changing the conceptualization of misattribution does not by itself address the issues raised here.

On the other hand, it may be that misattribution is not the mechanism, which mediates AMP effects. This possibility would have significant implications for a variety of theories and methods that rest on this idea. For instance, it would seriously challenge the implicit misattribution account of AMP effects, which currently dominates the AMP literature. It would call into question recent theoretical perspectives on misattribution that rely on the AMP for support. This includes theoretical models relating to the process of misattribution itself (e.g., the process model of misattribution: Payne, Hall, Cameron, & Bishara’s, 2010), as well as claims that evaluative conditioning is based on a misattribution process (Jones et al., 2009), and that psychological properties beyond evaluations can also be misattributed (Blaison, Imhoff, Hühnel, Hess, & Banse, 2012). It would also call into question a number of second-generational tasks that attempt to exploit the misattribution of meaning (the Semantic Misattribution Procedure: Sava et al., 2012) and truth (the Truth Misattribution Procedure: Cummins & De Houwer, 2019). Such measures have themselves been used to investigate psychological phenomena like gender stereotypes (Ye & Gawronski, 2018), sexual preference, (Imhoff et al., 2011), self-concept (Wong, Burkley, Bell, Wang, & Klann, 2017), and personality (Sava et al., 2012). It seems likely that the very same issues associated with influence-awareness in the traditional AMP are likely to play similar roles in these other procedures. Future work could employ a similar IA-AMP-style manipulation to these variants to investigate this issue in more detail. In short, our findings call into question several existing theoretical perspectives on misattribution.

**Practical implications: Can we continue to use the AMP?** Imagine that we set the AMP’s status as an *implicit* measure to one side and merely ask the question: does the task have utility as a measure of attitudes in general? Our findings suggest it does not. One of the most pressing issues raised by Experiments 1-5 is that instead of capturing general processes taking place in the general population, AMP effects seem to reflect a subset of influence-aware trials, especially in highly influence-aware participants who are consistent across AMPs. In other words, AMP effects are a poor index of ‘general’ evaluations in groups of people and a good measure of evaluations in highly influence-aware people (who make up a minority of individuals in the task). Such a finding suggests that scores on the measure do not reflect what most researchers assume or desire. This is highly problematic for its use in both basic and applied settings.

To illustrate, imagine that a researcher is interested in assessing levels of implicit racial bias in law enforcement officers. She administers a race AMP to police officers, finds evidence of a large mean AMP effect for the group, and subsequently infers that police officers are, in general, implicitly biased against this racial group. Our findings suggest that the AMP effect is not capturing racial bias in populations *in general*, but rather the performance of a select number of participants who are highly aware that race-related primes were influencing their responses to the target stimuli. Importantly, Experiment 4 demonstrates that these participants are likely to demonstrate AMP effects regardless of the domain being assessed. This is obviously neither what is inferred from such studies nor what the researchers intended to capture. In other words, most researchers who employ the AMP are interested in a given population’s (implicit) evaluations. This is not what the task seems to measure.

Just as our findings suggest that the presence of an AMP effect at the group level is not reflective of a general process in the general population, they also suggest that the AMP effect of a given individual (or lack thereof) is not diagnostic of that individual’s evaluations. To illustrate, consider our previous example of implicit racial bias in police officers. Upon administering a race AMP to a specific police officer, the researcher observes that this particular officer displays an entirely neutral AMP effect (i.e., they evaluate targets as equally pleasant when preceded by a Black face or a White face). The researcher concludes that this officer is less biased against black people compared to his contemporaries who, on average, demonstrate moderate anti-Black AMP effects. Yet our findings suggest that the neutral AMP effect observed in this officer does not mean that the officer has no particular racial evaluations. It may be the case that the officer holds very strong anti-black evaluations but does not produce an AMP effect due to his low influence-awareness rate. If so, then the researcher’s conclusions may be inappropriate. In short, our findings suggest that the absence of an AMP effect cannot be used to infer the absence of evaluations, which raises questions about the validity of the AMP itself.

## Future Research Directions

**Creating a better implicit measure**. One could argue that the AMP should be modified in order to exclude influence-aware trials, or be combined with a manipulation that diminishes the role of influence-awareness on task performance. These changes would allow it to maintain its status as an implicit measure. Our results could be seen to support this claim given that (a) even those participants with influence-awareness rates of zero demonstrated (very small) IA-AMP effects, and (b) IA-AMP effects calculated from non-influence-aware trials still possessed some predictive validity for discriminating between known groups. In other words, researchers may be tempted to set the traditional AMP to the side, employ an IA-AMP, exclude all influence-aware trials, and calculate an effect. This is certainly one way forward. Yet we see several immediate issues.

First, one should not conflate ‘*non*-influence-aware’ responding with ‘influence-*unaware’* responding. The IA-AMPs used here asked participants to press the spacebar if their target evaluation was influenced by the prime. The presence of such a response provides a measure of influence-awareness. Yet the absence of such a response is far more ambiguous. It may be that such trials are free from influence-awareness (i.e., are ‘*influence-unaware*’), or they could equally reflect uncertainty about influence, momentary distraction, or other sources of control over responding. Thus caution must be exercised when assigning a specific meaning to non-responses in the IA-AMP. Were a researcher to merely exclude influence-aware trials from the IA-AMP, the remaining non-influence-aware trials would not necessarily provide a measure of influence-unaware evaluations. To arrive at such a conclusion, one would need to develop and test a hypothetical ‘Influence-Unawareness AMP’ (IU-AMP): for example, by asking people to respond when their evaluation was *not* influenced by the prime. The key point here is that such an ‘Influence-Unawareness AMP’ should not be conflated with our ‘Influence-Awareness AMP’.

Let us take a step back and assume that such an IU-AMP could be developed, and would be capable of capturing influence-unaware effects. There are likely two different groups of researchers that would employ the measure (as these are the very same groups who use it now; Payne & Lundberg, 2014): (a) applied researchers who wish to use an implicit measure in a specific domain (e.g., of clinical or social phenomena), and (b) basic researchers interested in the idea of automaticity (i.e., those studying implicit processes). In either case an IU-AMP would not be without issue. Imagine that an applied researcher in a specific domain wishes to examine differences between two known-groups using the IU-AMP and obtains results similar to what we report in Experiment 4 (*d* = 0.62, using IA-AMP non-influence-aware trials for comparison). To appropriately power her study using the IU-AMP to detect group differences would require at least 138 participants.[[11]](#footnote-14) For the applied researcher, collecting such sample sizes is often either unfeasible or a poor use of limited resources. In contrast, if predictive utility was more important to her than ‘process purity’, then an IA-AMP capturing influence-aware responses could detect group differences with as few as 16 participants (i.e., IA-AMP effects calculated from influence-aware trials: *d* = 2.08).

Now imagine the other case. For basic researchers, the need to collect larger sample sizes may be both feasible and desirable if this allows them to study implicit processes in a relatively ‘pure’ manner. The problem here is that an IU-AMP will likely also lead to a significant number of people being discarded due to zero, or near-zero, levels of unaware task responding. The implication here is that although such an IU-AMP might provide a better implicit measure by implementing changes to the task, the effects obtained from such a task would not reflect behaviors (or mental processes) in people *in general*. Yet this is exactly what the AMP is primarily used for. Therefore, just as other fields acknowledge the variety of issues associated with making inferences or generalizations about people in general from non-representative samples (e.g., WEIRD individuals: Henrich, Heine, & Norenzayan, 2010; neuroscience tending to only study the brains of right-handed people: Willems, der Haegen, Fisher, & Francks, 2014; animal models of pathology that are based on male biology but not female: Mogil, 2016), we need to do the very same. Both applied and basic researchers using the AMP (or AMP-like tasks, including the IU-AMP) need to carefully attend to the dangers of making inferences and generalizations about people in general from special subsets of people.

Even if one were to take these steps to obtain an ‘implicit’ effect, they would still have to grapple with the fact that there is good reason to believe that such an effect would not represent a structurally-valid measure of evaluations. Structural validity assessments (*see above*) indicate that performance on non-influence-aware IA-AMP trials lacks structural validity. Furthermore, the AMP is not structurally sound unless participants demonstrate high rates of influence-awareness (i.e., > 66%). This is the case for the vast majority of individuals in our studies (79%). Thus, ‘implicitness’ aside, this raises the question of whether this new AMP would even represent a valid measure of evaluations at all.

**Revision of existing findings.** Assuming we are correct, our findings suggest there is a pressing need to revise many of the conclusions made in the AMP literature. These conclusions are typically made on the basis of two common assumptions: (a) that AMP effects are reflective of implicit attitudes, and (b) that AMP effects represent an equally valid measure of such attitudes across all individuals (e.g., Fox et al., 2018; Kalmoe & Piston, 2013; Mann et al., 2019; Payne et al., 2005; Rinck & Becker, 2007; Spring & Bulik, 2014). To illustrate, consider a study by Franklin, Puzia, Lee, and Prinstein (2014), which concluded that “young adults with a history of non-suicidal self-injury (NSSI) display a significantly stronger *implicit* identification with [images of skin] cutting” compared to their counterparts without such a history of NSSI. Our findings suggest that such a result should instead be interpreted as “in those young adults who are highly influence-aware on the AMP, those with a history of NSSI also self-identify more with NSSI compared to those who had no such history. However, little can be said about those with low influence-awareness rates.” This is just one example; similar revisions need to be applied to the core claims of all published research using the AMP (e.g., via systematic review), which may fundamentally alter the conclusions derived from that body of work.

**What makes a person influence-aware?** Our results also raise the question of what characterizes and differentiates highly influence-aware participants who drive AMP effects from the rest of the population? Our results from Experiment 5, which employed the modifications to the AMP suggested by Mann et al. (2019), suggest that the rate and impact of influence-awareness on AMP effects is not reduced through simple alterations to the task itself. Results from Experiment 4 suggest that an individual’s influence-awareness rate is consistent across IA-AMPs assessing different domains, and that the influence-awareness rate demonstrated in one domain predicts AMP effects in another. As such, it seems that influence-awareness rates are an individual difference variable rather than merely random noise or properties of the task itself. While beyond the scope of the current research, future work may wish to examine whether influence-awareness is a state- or trait-like property (e.g., whether it is consistent across time and context), whether it is related to other individual differences (e.g., Need for Cognition: Cacioppo & Petty, 1982), or indeed whether influence-awareness on the AMP is related to performance on other kinds of implicit measures (e.g., the Implicit Association Test).

**Diversity and Inclusiveness**

Experiments 1-5 were carried out online on a platform that recruits participants from the general population (Prolific Academic). An analysis of the demographic data we requested in our studies (age, gender, and political orientation [in Experiments 3 and 4]) revealed that our samples were broadly representative in terms of age (ranging from 18 to 65 years), and balanced in terms of gender (547 women, 469 men). As such, in our efforts to reexamine different properties of the AMP effect, we recruited samples that were, at worst, no less representative as the original AMP studies that we build and extend upon. At best, our samples are likely more representative than the original studies given that we did not recruit from undergraduate students (i.e., more balanced in terms of gender, broader age range). That said, we did not request information on other demographic variables (e.g., sexuality, ethnicity). Although we had no theoretical reason to assume that such variables would moderate performance on a generic positive/negative AMP, it is perhaps plausible they are associated with performance on the political (IA-)AMPs. Likewise, given that Experiments 3-4 were sampled exclusively from US residents, and that the majority of Prolific Academic participants reside in the UK, our samples are primarily made up of (and potentially over-represent) individuals from these nations. Future work may therefore wish to capture more detailed demographics information or could replicate our findings across different nationalities to further expand its remit.

## Limitations

One might argue that we did not effectively capture the behavior we were ultimately interested in (i.e., awareness of prime influence). For instance, Payne et al. (2013) argued that assessing awareness of prime influence *after* a response has been emitted is suboptimal because participants may confabulate reasons for their performance in a post-hoc fashion. Although we tried to control for this by assessing awareness after each trial rather than after the task, others could argue that such a measure still reflects post-hoc confabulation. Post-hoc confabulation could indeed explain why influence rates predict AMP scores withinthe same AMP, however it cannot explain why influence rates assessed in one AMP were equally predictive of AMP scores in a previously-completed AMP assessing an entirely different domain (see Figure 3). Such a finding suggests that people were not simply confabulating reasons for their responses. Rather, our measure was sensitive to their awareness of the prime’s influence on their responses.

## Conclusion

Our findings indicate that AMP effects are not implicit (in the sense of unaware) and pose a direct challenge to the idea that they are mediated by misattribution. We also demonstrate that previous literature on the AMP effect’s unintentionality contains many statistical, methodological, and conceptual issues that weaken its conclusions. Together, this suggests that the current evidence does not suggest the AMP effect is implicit (i.e., there is no clear evidence for being unintentional, and new evidence against being unaware). This leads to a host of conceptual, theoretical, and applied issues for past and future research using the AMP to make inferences about psychological phenomena. Finally, our findings indicate that AMP effects do not represent an equally valid measure of attitudes across individuals. Thus, taken together, it appears that what is useful about the AMP effect is not particularly implicit, and what is implicit is not particularly useful.

It may be the case that, as a field, our collective use of the AMP bears similarity to the parable of The Emperor’s New Clothes (Andersen, 1837). In that tale, the emperor is supposedly clothed in the finest attire, but during a procession his subjects gradually recognize that this is not the case. Our findings suggest that the AMP may not be clothed in the automaticity condition that we previously assumed it was. In the original tale, the emperor realizes his folly, but insists that the procession go on. We believe that proceeding as normal with the AMP without acknowledging what our findings imply and adjusting our beliefs and practices accordingly may be an equal folly, and one that serves to hamper rather than advance both the measure’s use and our understanding of the phenomena we are ultimately interested in.

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1. There is extensive debate about the meaning and usefulness of the construct of “implicit” (e.g., Corneille REF etc). However, the idea that the AMP captures unintentional and unaware behaviour (which is often labelled as “implicit”) is at the core of its use. [↑](#footnote-ref-1)
2. In our pre-registration we misspecified this as the ‘proportion of influenced to non-influenced trials’ (i.e., a ratio rather than a proportion). We opted to instead influenced trials as a proportion of the total number of trials. [↑](#footnote-ref-5)
3. For the sake of readers who may be less familiar with how ‘big’ a given Odds Ratio is, we approximately convert all Odds Ratios to comparable Cohen’s *d* values using the method proposed by Hasselblad & Hedges (1995, Equation 5, p. 170; see also Sánchez-Meca, Marín-Martínez & Chacón-Moscoso, 2003). [↑](#footnote-ref-6)
4. For readers who are interested in comparisons between this interaction effect and the main (IA-)AMP effects, comparisons between these are made in the meta analysis section below. [↑](#footnote-ref-7)
5. More participants exist than specified in our pre-registration due to an error in how exclusions were originally implemented in our data processing R script. Data collection was stopped when we believed we had 150 participants, as per the pre-registration. A code review revealed that some participants were erroneously excluded. The final analytic sample therefore includes these participants. [↑](#footnote-ref-8)
6. Technically, this relationship can be referred to as ‘postdiction’ given that the DV was completed prior to the IV. However, for reader familiarity we adopt the term ‘prediction’ throughout. [↑](#footnote-ref-9)
7. Our pre-registration stated that this hypothesis would be assessed via a paired-samples *t*-test (reported in the Supplementary Materials on OSF). However, we neglected to consider that participants who demonstrated an influence-awareness rate of 100% would therefore have no trials to calculate an IA-AMP effect from for this analysis. Simply excluding these participants would not be appropriate as these participants’ AMP effects are highly relevant to the hypothesis. Instead, we employed a Partial-Overlap *t*-test, which can be used in cases where some data is dependent and some is independent (Derrick, Toher & White, 2017). Cohen’s *d* values are reported for consistency however, as no partial-overlap version of *d* exists to our knowledge, these results should be interpreted with caution, and its confidence intervals not be used for decision-making. [↑](#footnote-ref-10)
8. Our pre-registration stated that we would compare differences between these conditions via the confidence intervals on the two Cohen’s *d* estimates. We subsequently discovered a method to produce a *p* value for this comparison via the metafor package’s heterogeneity test. Both are reported, and results are congruent among them. [↑](#footnote-ref-11)
9. This scoring method has been employed at both between participant analyses (where AMP effects were first scored using this method) and within participant analyses (if implicitly, as the multilevel models examined moderation of the rating ~ prime type effects). [↑](#footnote-ref-12)
10. This analysis was not pre-registered: the assumption we test and the method of testing it only occurred to us after data collection. [↑](#footnote-ref-13)
11. Using G\*Power (Faul, Erdfelder, Buchner, & Lang, 2009): Independent *t*-test, alpha = 0.05 (two-sided), power = 0.95. [↑](#footnote-ref-14)