Editor’s comments:

As you can see when you have had a chance to see the reviewer comments, we all find this line of inquiry to be promising and a valuable contribution to the field. The AMP is widely used, so we all appreciate the goal of taking a careful look at how it operates and the degree to which awareness of the primes influences the focal effect.

At the same time, we all note substantial weaknesses with the study, particularly with regard to alternative explanations of your results and how well your paper is positioned to contribute meaningfully to previous conversations about the validity of the AMP.

I am sorry to report that I cannot accept the current version of the paper for publication in JPSP-ASC. However, in recognition of the potential value of this line of work to researchers in the field, I would be willing to entertain a substantial revision (submitted as a new manuscript) if you believe it is possible that a revision would be able to address the reviewers' critiques.  
  
The reviewers clearly expressed their concerns and thus I will not reiterate them. However, let me highlight a few points that are most important. First, both reviewers pointed out specific ways that previous work (e.g., Payne et al., 2015 and Gawronski & Ye, 2015) was mischaracterized. Usually, miscommunicating details of the method of previous work would not be considered a major flaw of a paper, but it is quite troubling to see in a paper whose raison d'etre is to identify flaws in an experimental procedure used by other researchers. In this case, a fair scientific debate demands that the critic be accurate and specific about exactly what the flaws of the prior work are.  
  
Second, both reviewers also point out that mere awareness of the primes does not imply that the awareness "drives" the focal AMP effect. (As a side note, we all found the use of the term "drive" throughout to inappropriately imply a causal effect.) The reviewers are quite articulate about the issues here, with the overarching conclusion being that a causal effect of awareness is only one among many possible explanations for the AMP effect. The alternatives are not adequately ruled out and, as Reviewer 2 notes, all the observed effects in this paper are consistent with implicit misattribution plus some post-hoc justification. The evidence presented here simply does not support the conclusion that the AMP is invalid in the ways you claim (or even really in any ways), so even as the general undertaking of interrogating the AMP might be valuable, the specific way it was implemented here does not contribute much to the literature.  
  
Finally, I want to add a note about what I would expect in a revision. As noted by both reviewers, many of the issues raised here have already been hashed out in the previous back-and-forths about the AMP. And as I wrote, the data do not warrant sweeping conclusions such as the title's provocative implication that the village of people who use the AMP have collectively decided to ignore its naked absurdity. But, some valid questions do remain about possible limitations of the task, when and how awareness matters (or not), and some detailed mapping of boundary conditions regarding when and for whom misattribution is more or less likely to occur. I think a careful, measured interrogation of these issues would be quite valuable to researchers and make a solid contribution to the literature. I would welcome it at JPSP-ASC if you believe you can make such a contribution.  
  
Reviewer #1 comments:   
  
In a meta-analysis of the five experiments, the authors reported that 54% of participants reported priming in 0-20% of trials, 14% reported priming on 21-40% of trials, 8% reported priming on 41-60% of trials, 6% reported priming on 61-80% of trials, and 17% reported priming on 81-100% of trials.

In another meta-analysis of the five experiments, the authors reported that the average rating of the targets after positive primes and the average rating of targets after negative primes were positively correlated when computing those averages only from trials in which participants did not report a priming effect, and were negatively correlated in trials in which participants reported a priming effect.  
The authors concluded that the priming effect in the AMP is not implicit: "there is no clear evidence for [the priming effect] being unintentional, and new evidence against being unaware".  
  
1. The manuscript has a great potential to make a positive contribution to the scientific community. The main strength of the manuscript is the finding that reporting the priming effect in one AMP predicts the priming effect in a previous AMP. Other informative findings are the conceptual replication of the positive relation between the priming effect in the AMP and retrospectively reported priming, and some evidence that might suggest that good psychometric qualities in the AMP depend on a minority of the participants - those who report the priming effect. The authors also provide an interesting discussion of previous results and interesting strong opinion on how these findings should influence researchers who use the AMP.  
This manuscript is a clear challenge of the validity of the AMP, and researchers should be exposed to that challenge, to help them decide whether to use the AMP, and how to interpret results obtained with the AMP. Personally, my conclusion about the AMP has not changed: it is one of the best indirect measures of evaluation we have, but that's only because we do not have good measures. Like the other implicit measures, its validity is highly questionable, and inference from results obtained with the AMP is currently very tentative. I agree with the authors that many publications do not seem to exercise the appropriate caution when interpreting AMP results, and I believe that this manuscript could help raise awareness about the possible weaknesses of the AMP. It will be highly cited and could have a very positive impact on people's understanding of the AMP.  
  
Notwithstanding the great potential of this manuscript, it has some weaknesses that might damage the readers' understanding of current evidence about the AMP. In the rest of this review, I will list a few comments and suggestions that the authors might consider in a possible revision, all with the purpose of improving the service the manuscript would provide to the readers, and minimizing possible negative effects.  
  
  
2. The authors argue that retrospective awareness of the priming effect suggests that misattribution does not underlie the mechanism. They argue that misattribution requires unawareness. This seems logical: if one is aware of a misattribution, then one can correct that misattribution before responding. However, this is not definite. First, awareness might have risen only after observing the response. In fact, awareness might not occur at all unless prompted with the direct question about the priming effect. Second, participants could suspect that the prime influenced their evaluation of the target even before they rate the target, but without any choice other than evaluating the target, there is little reason for them to reverse their response (e.g., from Pleasant to Unpleasant). In other words, in the AMP, participants cannot avoid misattributing even if they suspect that it occurred. More broadly, being able to detect misattribution does not mean that people know how to correct for it. Thus, I am not sure that what the authors present as the most likely conclusion from their findings (misattribution does not underlie the priming effect) is the only possible conclusion. It is definitely a plausible conclusion - plausible enough to cast serious doubt on the AMP's validity, but readers would benefit from exposure to other possible conclusions.  
  
3. Still on the same subject, in the modified AMP, participants could use the compatibility between the valence of the prime and the valence of their rating as evidence for the influence of the prime on the target. Therefore, even if participants have no awareness of the priming when it occurs, they could still respond based on that compatibility. Further, it seems reasonable that people would detect a compatibility between their rating of the target and the valence of the prime more frequently when participants are more sensitive to the AMP (e.g., to misattribution). In other words, if some participants are more likely than others to show priming in any AMP, they would also be more likely to report the priming (in any AMP). Therefore, the finding of a positive relation between the awareness in the modified AMP and the priming effect in another non-modified AMP is not unequivocal evidence that misattribution is not responsible for the priming effect in the AMP. Again, the authors' account is plausible and important to share because it has serious implications, but the readers would also benefit from an explicit reminder of alternative accounts.  
  
4. Related to the previous point, in p. 15, the authors wrote that they sought to determine if awareness drives AMP effects. They then use the verb "driven" often throughout the manuscript. I think that "drive" implies a causal role for awareness. However, the authors did not manipulate awareness. Therefore, they can conclude only about the possibility of a relation between awareness and the AMP effect, and not a causal relation. Very often, the word "drive" seemed inaccurate and might have conveyed the wrong message. Often, moderation of the priming effect by reported priming was described as evidence that the priming effect was driven by awareness or by trials in which participants showed awareness, or by participants who reported much awareness. It is possible that I do not understand the meaning of "drive", but I do not think that it is common to describe findings of moderation, especially when the moderator is not manipulated, as evidence that the moderated effect is driven by the moderator.  
  
  
5. The authors seem to accept the idea that in order to measure implicit cognitions (e.g., attitudes that influence behavior without people's awareness), the mechanism that underlies performance in the measure must be implicit (e.g., the priming effect in the AMP must occur without people's awareness). Clearly, this is not always the case for psychological measures. When I report that I strongly agree with the statement "I am shy" in a shyness questionnaire, it is likely that none of the processes that cause my shy behavior also cause my response in the questionnaire. This might be also true for the IAT and evaluative priming: it is possible that the processes the mediate the effect of mental associations on performance in those tasks are quite different from the processes that mediate the effect of mental associations on automatic evaluation.  
  
The authors might argue that if the priming effect in the AMP elicits awareness, there is little reason to suspect that the AMP would measure evaluation that escapes awareness. That might be so, but, by now, there is published evidence about the validity of the AMP as a measure of automatic evaluation that go beyond the investigation of the processes that underlie the priming effect in the AMP (for reviews, see Cameron,Brown-Iannuzzi, & Payne, 2012; Payne & Lundberg, 2014 [see the validity section]). It would benefit the readers if the authors acknowledge that. The authors could also choose to review that evidence and cast doubts on their validity (e.g., I have not seen any convincing finding that was replicated in an independent lab). Yet, at this time, even a finding that the priming effect in the AMP is completely intentional would not suffice for the conclusion that it is not a good measure of automatic evaluation, without arguments against the evidence reported so far from (mostly correlative) validation studies that helped establish the AMP as a measure of implicit social cognition.  
  
  
6. The description of Experiment 2 in Payne et al. (2013) does not seem accurate. To the best of my understanding, the most important finding was that the AMP predicted judgment of a Black (but not White) target that behaved ambiguously, whereas the direct rating of the primes did not. I think that this is one of the best findings in support of the AMP as a measure of an implicit construct (and pursuing its replication should be a priority of our field, especially considering the rather small sample in the original experiment, n = 45). In the first description of this experiment in the present manuscript (pp. 8-9), that aspect of the experiment is not mentioned at all. Later (pp. 13-14), the authors wrote that Payne et al. "based their inference on the fact that there was a significant difference between personality judgments and 'intentional' AMP effects, but no significant difference between personality judgments and 'unintentional' AMP effects". But it is unclear what they mean by "difference". The test in question was of a relation between the AMP effects and the personality judgment, not of a difference between them (it would also be unclear to the readers what the authors mean by "personality judgments" because this aspect in the experiment is never described in the present manuscript).  
  
7. The description of the results and conclusions of Experiment 3 in Payne et al. (2013) do not seem accurate. The authors wrote: "Even though there was no way to determine what proportion of AMP effects were driven by aware vs. non-aware trials (given the necessary data was not collected), the authors still argued that effects on the traditional AMP did not differ from those on the modified AMP, and used this as evidence for the relative unawareness of the AMP."  
  
First, the comparison between the AMP with and without the option to skip trials in which the participant suspect a priming effect is informative. Had Payne et al. (2013) found a reduction in the priming effect in the modified AMP, in comparison to the traditional AMP, that would have supported (to some extent) the argument the priming effect in the AMP requires awareness. Surely, under NHST, lack of significant evidence is less definitive than finding significant evidence, but that is not related to the lack of appropriate comparison (further, Payne et al. addressed the issue of statistical power in their discussion of the results of that experiment, p. 383).  
  
Second, and perhaps more important, the authors ignore a major finding in Payne et al.'s (2013) Experiment 3: "Participants passed much less when the primes were pleasant (M = 0.14) or unpleasant (M = 0.17) than when the prime was neutral (M = 0.54), F(2, 70) = 28.23, p < .001. Passing rates on neutral trials were significantly higher than pleasant  
trials, F(1, 35) = 34.0, p < .001, or unpleasant trials, F(1, 35) = 25.65, p < 001". Clearly, that pattern is the opposite of real awareness of the priming effect. Why would there be more priming when the prime was neutral rather than of clear valence? Payne et al. (2013) proposed a plausible explanation: when priming occurs, participants feel (because of misattribution) that they have clear evaluation of the target. When priming does not occur, participants are less convinced regarding their evaluation of the target, and are more concerned that the prime influenced that evaluation.  
  
8. To conclude points 5 and 6, the weaknesses the authors found in Payne et al.'s (2013) research are not very convincing, and also seem to rely on inaccurate or incomplete description of Payne et al.'s studies. As a slight digression, I would add that this flaw in the present manuscript is unfortunate because Payne et al.'s (2013) studies had several weaknesses. In Experiment 1, the fact that some participants reported unintentional rating of the primes does not preclude the possibility that other participants rated the primes intentionally (i.e., perhaps those who report intentional and those who report unintentional priming are not the same people). For Experiment 2, if the priming effect is driven mostly by a minority of participants who choose to intentionally rate the primes, then the AMP is not exactly the same measure as a direct rating of the primes. For instance, perhaps, unlike direct rating, most of the variance in the AMP comes from people who do not try to hide their preference for one social group over the other. That difference between the AMP and direct rating of the primes could be the reason why the AMP is sometimes better than direct rating in predicting race-related behavior. For Experiment 3, if the priming effect is driven mostly by a minority of participants who choose to intentionally rate the primes, then it seem likely that these people would not want to use the option to pass trials in which the primes influence their rating of the targets. As a result, that modification of the AMP would not be effective in eliminating intentional rating of the primes.  
  
  
9. It was not entirely clear what methodological shortcomings Gawronski & Ye's (2015) research had. Their crucial finding was that the retrospective reports of the priming effect correlated with the priming effect only for the topic that was salient during the task, and not for the topic that was not salient. If the reason for the correlation between the priming effect and retrospective reports of the priming is due to intentional rating of the primes, why would the manipulation of topic salience influence this correlation without influencing the priming effect itself? The present authors wrote "retrospective self-reports do not provide a direct assessment of the construct under investigation". Yet, Gawronski and Ye did not rely on those self-reports as a measure of awareness of the priming effect. Rather, they tested whether the finding of a correlation between retrospective self-report and the priming effect survives a certain manipulation of awareness. They showed that their manipulation of awareness decreased the validity of the self-reported awareness of the priming effect as a predictor of the priming effect but did not decrease the priming effect itself (the results summarized in Table 1 in Gawronski & Ye's article are the best evidence I have seen so far, against the intentional rating account). It seems reasonable to conclude from that evidence that the self-reported awareness of the priming was not due to a necessity of awareness for the priming effect to occur.  
  
10. The authors conclude that the AMP priming effect "do not represent an equally valid measure of attitudes across individuals". This seems a valid conclusion from the evidence they report, and it is compatible with the evidence reported in Bar-Anan & Nosek (2012, 2014). In our 2012 research (mainly in Tables 3 and 4), we showed that indices of psychometric quality are reduced when excluding from the analyses participants who reported intentional rating of the primes (or, at least, awareness of the priming effect). We also found (see Appendix D of Bar-Anan & Nosek, 2014, Figures A and B, at <https://static-content.springer.com/esm/art%3A10.3758%2Fs13428-013-0410-6/MediaObjects/13428_2013_410_MOESM1_ESM.pdf>) that the AMP loses its relation with direct measures of evaluation much faster than other indirect measures, after removing participants with extreme scores (those with the largest priming effects). However, all that evidence is still insufficient to inform us how serious this problem is. Only the appendix from our 2014 paper provides some comparison with other indirect measures (and the AMP seems inferior to the other measures). Yet, I did not see much research about how many participants "drive" typical effects in social psychology, and how many are the main contributors to validity evidence of psychological measures. I also do not know of much research that informs us how inequality in validity of a measure across individuals affects scientific progress. Clearly, it is better if a measure works well for a larger portion of the population, but what is the standard and how much does scientific progress suffer from each drop in that equality? I think that readers would need that knowledge in order to make strong conclusions about the implications of the inequality reported in the present manuscript.  
  
  
11. In the "Structural Validity" section, the authors seem to expect a negative correlation between rating of targets after positive primes and rating of targets after negative primes. That would be the case mostly if priming is the main factor that influences the rating of the targets. However, there might be other factors that influence the rating of the targets. If that is the case, then controlling for those factors would be useful for a better measurement of the construct reflected by the priming effect. By comparing two categories of prime stimuli (e.g., positive and negative primes), one can minimize the effect of non-evaluative factors that influence the rating of the targets (e.g., liking of the Chinese culture, and a general tendency to rate stimuli as positive or negative). In other words, the measure of evaluation in the AMP is not the average rating of the targets after a certain category of primes. It is the comparison between the average ratings of the targets after one category of primes and the average ratings of the targets after another category of primes.  
  
For that reason, I did not accept the authors conclusion that "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. " (p. 53).  
  
12. Somewhat related, I do not think that the authors were accurate when they wrote that "the primes only exert influence on ratings within the AMP task when participants are highly influence-aware." Figures 2 and 3 suggest that priming occurred even when participants report no awareness of the priming effect. Further, although throughout the manuscript the authors often did not report the priming effect in "unaware" trials, whenever they reported that effect, it was significantly larger than zero (in p. 29, the effect was d = 0.82; in p. 38, the effect was d = 0.62).  
For a similar reason, I think that the authors are inaccurate to conclude, in p. 56, that for the majority of participants, scores cannot be said to represent a sound measure of  
evaluations at all. Unless I am missing something, Figure 3 seems to suggest that most participants show the priming effect, which reflects evaluation.  
  
  
  
13. In p. 21, when the AMP is first described in the method, I recommend providing more information about the procedure (trial sequence, block sequence, and procedure sequence) rather than refer the readers to a different paper.  
  
14. In p. 21, I was confused by the authors' description of the most crucial modification of the AMP: "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)." It seems that the instruction that appear to describe the request to respond to every trial is the instruction relevant to the awareness question. I had to read the Inquisit script (provided in online materials) to make sure I understood the task correctly.  
  
  
15. It would probably be helpful to most readers, if the authors provide clearer descriptive statistics for all their studies. In each experiment (and not only meta-analytically), I was particularly interested in the mean and SD priming effect for "unaware" and "aware" trials (and perhaps more details about the full distribution), the mean and SD number of "aware" trials, and a scatter-plot showing the relation between the percentage of "aware" trials and the priming effect in the same IA-AMP, and in the other AMP (Experiments 2-5). With those descriptive statistics, readers would have a much better understanding of the findings, beyond the results of the statistical tests.  
  
16. I applaud the authors for pre-registering their experiment and providing full access to their materials, data, and analysis. It is important to publish papers that follow these new norms. However, I was unable to find clear reports of the analyses that, according to the authors, were supposed to appear in the Supplementary Materials on OSF (e.g., footnote 8, a few times in p. 32, and once in p. 36). Perhaps the authors mean that these results appear in the html file produced by RStudio from the analysis scripts. I think that it would be better to provide a clear document (Word or PDF) with a summary of all the additional statistical analyses.  
  
  
17. In p. 45, the authors report the trial-level meta-analysis but refer the readers to Figure 2, which seems to show participant-level results.  
  
18. In p. 45, to interpret the moderation of the priming effect in each trial, by the self-reported awareness of the priming effect, the authors compared the moderation effect-size and the priming effect-size. That is interesting, but, usually, moderation is explained by reporting the simple effects in different levels of the moderator. In this case, it seems essential to report the priming effect in trials that ended with a space response (i.e., self-reported priming) and the priming effect in trials that ended without a space response (i.e., trials in which the participant did not report an influence of the prime on the rating of the target).  
  
19. P. 11: "Dietvorst and Simonsohn (2018) recently found that people readily incorporate to-be-ignored information into their responses on different tasks, despite the fact that researchers signal that this information was irrelevant and to be ignored". Does "readily" mean "intentionally"? If it occurs unintentionally, then this finding does not provide support for the authors' suspicion that participants ignore the instructions in the AMP, and intentionally use their evaluation of the primes when they rate the targets.  
  
20. In p. 28, the authors reported "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]." I assume they meant that reporting awareness of the influence of the primes moderated the effect of the prime valence on the target evaluation. This is not clear, currently. And, as noted earlier, moderation is not evidence that an effect is driven by the moderator. It is only evidence that the moderator moderates the effect.  
  
21. In p. 36, participants chose not to report in the main manuscript the results that replicated the relation between reporting priming and the priming effect (on the trial-level and on the participant-level). These results seem rather central to the present manuscript, so I suggest including them in the main text (if the results are complex or seem repetitive, a table might help).  
  
22. Experiment 4 provides an opportunity to examine whether reported priming equally predicts the priming effect in a subsequent and in a preceding AMP. In other words, it might be informative if the authors add the order of the tasks as a factor (and a moderating factor) in the multiple regressions reported in pp. 36-37. That would further test the bidirectionality of the relation between reported priming in one task and the priming effect in another task.  
  
23. In Figure 1, the labels were not immediately clear to me. The x-axis showed the priming effect, reflecting preference for Trump over Obama. The graph included labels to explain the meaning of the two most extreme possible scores (-1 and 1). However, those labels were not perfectly clear, and it was not clear that these labels were supposed to reflect the values -1 and 1. Instead of using those labels, it is common to simply explain, in the Figure's note, what a positive score reflects.  
  
24. I am not a native English speaker so I might be wrong. However, I thought it was odd to use the term "unaware psychological processes" in the Abstract. To the best of my understanding processes are not those with awareness. Minds have awareness. So minds can have awareness of processes. Similarly, I am not sure that the term "influence-aware trials" makes sense. But, perhaps it is the best abbreviated term to refer to "trials in which participants reported a priming effect."  
  
  
  
  
Reviewer #2:   
As the authors note in their literature review, this paper follows another paper by Bar-Anan and Nosek (2012) that took a similar approach to make similar claims. Those claims were rebutted by Payne et al (2013) and Gawronski and Ye (2014; 2015), who found that the evidence was consistent with a post-hoc confabulation account. That is, rather than accurately reporting the cause of their ratings, participants observed their responses and then reported whether they had been influenced (and if so, it must have been intentional). However, the authors argue that the present paper is different because whereas Bar-Anan and Nosek had participants complete an AMP and then give a holistic retrospective rating of whether they were influenced, the present paper asks participants to respond to the AMP on each trail, and then judge whether they were influenced by the primes on that trial. They argue (but do not provide any evidence) that the trial-by trial method is not vulnerable to post-hoc inferences.  
  
  
However, a fundamental problem for this paper is that this method is still a retrospective self-report. Trial-by-trial retrospective reports are used routinely to demonstrate post-hoc inferences of the type in question here. For example, Aarts, Custers, & Wegner (2005) used a trial-by-trial retrospective judgment to show that participants often falsely claim authorship over "decisions" made by a computer. Many other studies have used a similar immediate retrospective judgment (e.g., Wegner's I Spy study, Wegner & Wheatley, 1999). Another paper using immediate trial-by-trial retrospective reports to demonstrate post-hoc confabulations is Kühn and Brass (2009) which, strangely, is cited in this paper as evidence that unambiguous and immediate retrospective reports are likely to be accurate. In fact, that paper found that when people made impulsive errors in a stop signal task they often falsely claimed to have intentionally decided to make that choice. Kühn and Brass conclude, "Our data support the retrospective account of intentional action," (p. 12) based on the same kind of immediate retrospective reports used in this manuscript.  
  
  
The similarity between the immediate retrospective reports used in the present studies and the holistic retrospective reports used in Bar-Anan and Nosek (2012) should be clear from the fact that they are correlated so highly (r = .78).  
  
  
So why is it such a problem that the studies used retrospective self-reports that are vulnerable to post-hoc inferences? Statistically, this is an error known as "post-treatment bias" (Coppock, 2019; Montgomery, Nyhan, & Torres, 2018). It occurs when researchers use a variable that is affected by an experimental manipulation as a covariate or moderator to make inferences about the experimental effect. This creates a confound between the post-treatment variable and the experimental effect on any other outcome. In other words, this is a form of non-independent selection of the same form criticized as "voodoo" correlations by Vul et al., (2009). Concretely, if larger priming effects (the experimental effect of primes on ratings of pictographs) lead subjects to claim they are aware of the influence, then reported awareness can't be used as a meaningful moderator of the priming effect.  
  
  
Another way to look at this problem is that all of the analyses depend on the correlation between reports of awareness and the priming effect. The authors interpret their findings as evidence that people who show systematic priming effects have disregarded the instructions and intentionally rated the targets consistent with the primes. That is, aware and intentional ratings cause the priming effects. But all of the findings are just what the misattribution account predicts also. The misattribution account says that it is difficult to disentangle affective response to the primes and targets, so subjects often mistake the source of the affect as the pictograph target when it is actually the prime. (A misattribution by definition can't be made with awareness or intention). Participants can observe their own behavior and notice if they are responding in prime-consistent ways. If so, they can report afterward that they were influenced by the prime (see Payne et al, 2013 for the same argument). This means that when priming effects are larger, subjects should report more influence of primes. If you divide subjects into those that reported large influences and those who didn't, then those who did not report influence won't have much priming because they have been selected to be that way. So these studies do not distinguish between the misattribution account and the authors' intentional/aware account at all.  
  
  
A related problem is that the authors confuse correlation for causation throughout the manuscript. When using reported awareness as a predictor or moderator of the priming effects, they routinely use causal language to say that awareness "drives" the priming effect. In fact, they say the priming effect was "driven by" aware subjects 142 times in the manuscript. If each time, the authors instead correctly wrote that larger priming effects were correlated with subsequent reports of awareness, the problems would be more transparent.  
  
  
Experiment 2 found that reports of awareness were correlated with priming effects on a previously completed separate AMP, and Experiment 3 found the same thing when the other AMP measured attitudes on a different topic. The authors say that this pattern can't be explained by post-hoc confabulations, but it clearly can. These effects also follow from the misattribution account. All implicit tests are indirect tests: they measure evaluations by how the evaluation perturbs performance on some primary task. This means that scores on implicit tests are influenced not only by the evaluation of the attitude object but also by performance on the primary task. This has been known for many years and is why much has been written about how implicit tests are not "process pure" (Jacoby, 1991; Payne, 2001). Various modeling approaches, such as multinomial models (e.g., process dissociation, quad model) have been developed to deal with this, including a multinomial model of the AMP that estimates component of performance by separating evaluations of primes from the likelihood of making misattributions (Payne et al., 2010). These findings simply show that individuals who make more misattributions show larger priming effects across different AMPs and that they also report being influenced by the primes. Again, it's just a correlation with a retrospective self-report. And it is predicted by the misattribution account of the AMP.  
  
More specific points.  
  
In the introduction the authors attempt to argue against some of the previous points made in the exchange between Bar-Anan and Nosek and Payne et al (2013) and Gawronski and Ye (2014, 2015). First, they argue that it is problematic that the AMP defines wat is intentional and unintentional by the instructions, and they note that sometimes subjects don't follow instructions and instead incorporate information that the researchers instruct them to ignore (p. 11). Subjects sometimes do this, of course, but the question at issue is why. Unintentional effects of primes on judgments is one reason they do so, although there are of course other reasons. Nonetheless, using instructions to define intentional responding is not a weakness. In fact, virtually every task that aims to measure performance by accuracy and errors must use instructions to define task goals and therefore what is accurate or error, and what is intended vs. unintended responding. For example in the Stroop task, experimenters must use instructions to tell subjects to name the font rather than read the words. Responses that diverge from the task goal (which is set by instructions) define automatic or unintentional behavior.  
  
  
Moreover, the paper never offers an explanation for why large subsets of subjects would choose to ignore the task instructions and instead intentionally rate the primes.  
  
  
Next, they argue that there are "statistical issues" in the Payne et al. (2013) paper. This section is full of factual errors. The paper says, "the authors found that the difference scores on 'unintentional' AMP and explicit race measures was larger than the difference between scores on the 'intentional' AMP and explicit race measures, and used this dissociation as evidence of unintentionality in the traditional AMP." But the Payne et al (2013) paper did no such thing. There were no comparisons between the size of difference scores with explicit measures. Next the manuscript says "Critically, however, the inference that 'intentional' AMP effects were "more affected" (p. 381) by the race of the prime than 'unintentional' AMP effects was never directly addressed in any of their other analyses…" and then go one to say we should have tested an interaction rather than reporting that an effect on one version of the test was significant and the other was not. But the present authors are entirely mistaken about the analyses we reported, and so their criticism is uninterpretable. That study examined the associations between two forms of the AMP (an indirect version in which subjects judged the pictograph targets and a direct one in which they were instructed to rate the primes) and impression judgments of a black or white target character (we examined main effects and interactions in a regression framework). And we tested the effect of seeing the black target character versus the white target character on indirect and direct AMP tasks. The hypothesis tested was that when people intentionally rate the primes their responses will be more reactive than the indirect version to the task they just completed. It is not clear how to respond to the statistical issues raised in this section given that the errors make it difficult to know what the authors are talking about.  
  
Finally, the authors note as a "conceptual issue" that in the 2013 study, "divergence from explicitly endorsed attitudes does not necessarily mean that the AMP captures unintentional behavior. Measures that are structurally dissimilar can show apparently unrelated effects due to the differences inherent in the measure" (p. 14-15). In the 2013 study, direct and indirect forms of the AMP were used, in which everything was held constant except the instruction to rate targets versus to rate primes. These direct vs. indirect forms of the task are actually the most structurally matched implicit-explicit comparison in the literature on implicit attitudes (we proposed this method in a 2008 paper entitled, "Why do implicit and explicit attitudes diverge? The role of structural fit"). So I don't know what the authors are talking about here.  
  
I don't normally comment on silly titles, but the reference to The Emperor's New Clothes implies not just that previous research with the AMP is mistaken, but that researchers in the field are fools for believing something that is obviously nonsense. This implication is gratuitously insulting, and suggests a lack of insight into the strength of one's own evidence.  
  
For the reasons described above, I don't believe the data reported here distinguish between the misattribution account and an aware/intentional account of AMP effects. I also don't believe they provide any new insight beyond the previous Bar-Anan / Payne / Gawronski exchange. Due to the basic error in using a retrospective self-report to make inferences about the causes of the priming effect that preceded it, I do not believe the data warrant publication. In retrospect, however, I am aware that it is possible that I may be biased.  
  
Signed,  
  
Keith Payne