

Review PCI Registered Reports

Is conscious perception necessary to direct attention? A replication of Jiang et al. (2006)

Dear editor and authors,

Thank you for the opportunity to read and offer my opinion for this most interesting manuscript.

The authors endeavour a very interesting Bayesian replication of Jiang and colleagues (2006), in which the original research team used null hypothesis significance testing (NHST) and found that interocular suppression using the Posner cuing paradigm provided evidence for *subliminally* directed attention to gender-of-interest naked images. The current authors use continuous flash suppression and provide initial, and suggest that they will provide conclusive, evidence that even if the Jiang and colleagues (2006) effect occurs, it is not in response to invisible/imperceptible/subliminal attentional direction. Their Bayesian statistical framework is excellently explained and – with a “young passion” and “experienced research confidence” that make me feel that both very promising and very senior researchers were involved in this work – their analyses is applied and will be applied to show this outcome.

I believe that the choice of replicating the Jiang and colleagues (2006) paper is an excellent one. I will start with what I find dire limitations in the original paper, that I believe the current authors could improve on.

Firstly, Jiang and colleagues confure the reader with their population sample. At first they quote:

“Results from 10 male and 10 female heterosexual participants revealed that invisible images did influence the distribution of attention.”

and then

“Ten heterosexual men and 10 heterosexual women participated in experiment 1. Ten gay men (an average score of 5.6 on the 7-point Kinsey scale; 0 is exclusively heterosexual, 3 is equally

heterosexual and homosexual, and 6 is exclusively homosexual) and 10 gay bisexual women (with an average Kinsey score of 4.5) participated in experiment 2”

It takes a lot to see that these two population samples are used in different experimental stages, there is no mention of whether the second sample of heterosexual participants was the same (or not) with the first stage and of course there is no mention of statistical power, which, anyway, would *embarrass* the paper with a less than optimal power co-efficient (e.g. $P_{(1-\beta)} \geq .8$).

Under this light, the replication could be arranged to amend this problem and improve the reliability of the results. I am sure the last author knows significantly more than me about Bayesian power, but I could simply contribute a few references as a courtesy:

Kruschke, J. K., & Liddell, T. M. (2018). The Bayesian New Statistics: Hypothesis testing, estimation, meta-analysis, and power analysis from a Bayesian perspective. *Psychonomic bulletin & review*, 25, 178-206.

Halsey, L. G. (2019). The reign of the p-value is over: what alternative analyses could we employ to fill the power vacuum?. *Biology letters*, 15(5), 20190174.

Dziak, J. J., Dierker, L. C., & Abar, B. (2020). The interpretation of statistical power after the data have been gathered. *Current Psychology*, 39, 870-877.

Stein, T., van Gaal, S., & Fahrenfort, J. (2023). How (not) to demonstrate unconscious priming: Overcoming issues with post-hoc data selection, low power, and frequentist statistics. *Low Power, and Frequentist Statistics*.

And of course:

Vadillo, M. A., Malejka, S., Lee, D. Y., Dienes, Z., & Shanks, D. R. (2022). Raising awareness about measurement error in research on unconscious mental processes. *Psychonomic Bulletin & Review*, 29(1), 21-43.

Also, as a topical researcher, I think Jiang and colleagues (2006) **explicitly** confuse responses before conscious awareness:

“However, it makes ecological and evolutionary sense if important events can influence observers’ spatial attention even before the observer becomes aware of the event.”

With responses **without** conscious awareness:

“Recent studies have shown that subliminal presentation of emotional stimuli can modulate activity of the amygdala (4, 5), a subcortical nucleus that is centrally involved in emotional information processing.”

This is a very common mistake, and perhaps the authors could benefit from the “ever-so-wise” literature that makes a distinction between these two, and dedicate a passage to this “oldest of fallacies”:

Bargh, J. A., & Morsella, E. (2008). The unconscious mind. *Perspectives on psychological science*, 3(1), 73-79.

Bargh, J. (2017). *Before you know it: The unconscious reasons we do what we do*. Simon and Schuster.

Bargh, J. A., & Hassin, R. R. (2021). Human unconscious processes in situ: The kind of awareness that really matters. *The cognitive unconscious*.

I have also written on that, but Bargh makes a much better argument for it, so I would go with him if you would like to address this issue.

Finally, concerning Jiang and colleagues (2006), I was always in awe of their statistical reporting:

“(positive attentional effect, $t_9 = 7.08$, $P = 0.0001$), (negative attentional effect, $t_9 = 2.41$, $P = 0.04$) and although “they did not show a significant attentional effect to invisible nude female pictures ($t_9 = 0.85$, $P > 0.4$)” (I am not sure what format $p < .04/p > .04$ is (!!!) “ $F_{1,36} = 32.3$, $P = 0.0001$ ”

I am not sure where they are getting their α thresholds (e.g., .04) and why there is not a single mention of effect sizes in the entire paper (e.g. Cohen’s d or η^2_p). I am pro Bayesian only replications and maybe now it is the time for the transition from NHST to Bayes **but** in this specific case – since the NHST statistical reporting is at best inadequate in the original – the authors might want to consider an Appendix section with proper reporting of NHST as well. After all, in my book, Jiang and colleagues (2006) failed to justify – even with NHST – their infamous conclusion:

“These results **clearly show** that spatial distribution of observers’ attention can be modulated by the presence of certain types of visual images even when the images are interocularly suppressed and invisible.”

Now, I find the current paper excellent, and I can only try to contribute to its improvement. Concerning the current paper then, in the abstract:

“...Observing attentional biases induced by visual stimuli below conscious threshold is one way of providing evidence...”

Do you mean:

“...Observing attentional biases induced by visual stimuli below **the** conscious threshold is one way of providing evidence...” or “...below the threshold of conscious awareness is ...”

I am sorry the text was available from the repository without lines, so bear with me while I am trying to offer my help. In page three, in reference “(Cohen, et al., 2012)”, I believe you have one too many commas. This happens again (e.g., “MacLeod, et al., 2002”) and then it does not happen in other occasions “(Wagenmakers et al., 2017)”. I am a bit confused, but my understanding is “(Evil-subliminal-researcher et al., 1666)”. I would say choose one format, if you know something I do not, and the extra comma is somehow correct.

Same page: “These approaches assume...” maybe “approaches” *per se* cannot assume, would you prefer “These approaches have been used to suggest” or a similar rephrasing?

References with three authors, such as “(He, Cavanagh, & Intriligator, 1996; Pitts, Lutsyshyna, & Hillyard, 2018)” can be written according to the new APA manual as “(He et al., 1996)” when in brackets, or “He and colleagues (1996)” when in text, if that helps you save word count in the paper.

In this passage, “...the functional nature of the different “access consciousness”” do you mean “...the functional nature of different accessibility levels to conscious awareness”?

Here:

“Whether the evidence for phenomenal consciousness rules out attentional involvement is debatable (Jennings, 2015; Phillips, 2011), it remains an influential idea (Cohen et al., 2012; Noah, & Mangun, 2020).” Are you missing a “but”?

“Whether the evidence for phenomenal consciousness rules out attentional involvement is debatable (Jennings, 2015; Phillips, 2011), **but** it remains an influential idea (Cohen et al., 2012; Noah, & Mangun, 2020).”

Struggling to make sense without the “but” there.

Similarly, “back-masking” is a tad odd, maybe the more traditional “backward masking” phrasing makes more sense?

There are a few instances like these above throughout the text. Would you care to proofread carefully and make your most interesting ideas more verbose and grammatically, syntactically, orthographically precise/correct please?

I do not wish to be pedantic, it is clear as day that your English is exceptional, so I will stop going point-by-point and leave it to you to apply your skills to improve the language in the text.

“In control trials in which the colour patterns were replaced by an identical pair of nude images to the other eye, so that the images were conscious, the attention modulation was non-significant.”

Concerning this argument, I am (again...) at awe that the subtext here is “unconscious priming works, conscious doesn’t” but if you would like to go deeper into it and comment it see:

Lapate, R. C., Rokers, B., Li, T., & Davidson, R. J. (2014). Nonconscious emotional activation colors first impressions: A regulatory role for conscious awareness. *Psychological science*, 25(2), 349-357.

And the rest of Regina’s (Lapate) work that – although I disagree with – makes a very similar argument.

Hmmm... let’s peruse together this passage: “On the theory that homophobic heterosexual men are unconsciously attracted to naked men, this group should show attentional attraction to naked male images; nonhomophobic heterosexual men will not show such attraction but rather repulsion.”

If you provide evidence that although d' , A' , A'' or A was not significantly different to chance-level perception, but you **prove** with Bayesian evidence that they were not at-chance (i.e., $BF > .3$) wouldn’t that mean any self-report was conscious and, therefore, subject to self-presentation sexual biases? What are you expecting if the responses involve conscious perception or meta-cognition (etc.)? Perhaps, any kind of psychophysiological assessment could “reveal” the truth here beyond self-reports, or if this is not possible to include a discussion

of self-reports vs psychophysiology is in order? For example, have a read at this, where the effect I am discussing is very-very clear:

Leong, M. Q., Yu, Z., Tsikandilakis, M., & Tong, E. M. (2023). “See no evil. Feel no evil?”: Exploring emotional responses to masked moral violations in religious and nonreligious Singaporean participants. *Evolutionary Behavioral Sciences*.

This is a – psycho-philosophically – very hard passage to write. I have highlighted it in many other papers and the reviewers asked for it more explicitly and in more depth. Mind the conservative reviewer here; they will ask for a distinction between self-reports, and implicit measures of arousal and repulsion. I am attaching some more papers where I addressed the subject to help you out. I am doing it to help you write this passage as best I can (though you are the better writers!). I am not doing for references:

Tsikandilakis, M., Bali, P., Derrfuss, J., & Chapman, P. (2020). “I can see you; I can feel it; and vice-versa”: consciousness and its relation to emotional physiology. *Cognition and Emotion*, 34(3), 498-510.

Tsikandilakis, M., Leong, M. Q., Yu, Z., Paterakis, G., Bali, P., Derrfuss, J., ... & Mitchell, P. (2021). “Speak of the Devil... and he Shall Appear”: Religiosity, unconsciousness, and the effects of explicit priming in the misperception of immorality. *Psychological Research*, 1-29.

Yu, Z., Bali, P., Tsikandilakis, M., & Tong, E. M. (2022). ‘Look not at what is contrary to propriety’: A meta-analytic exploration of the association between religiosity and sensitivity to disgust. *British Journal of Social Psychology*, 61(1), 276-299.

“Therefore, the theory concerning homophobia was not testable.

Indeed, it may not be testable even with a much larger N drawn from University of Sussex undergraduates.”

This is good, but it is speculative. I do think a discussion is in order, maybe visit the texts above and feel free to “borrow” a few ideas with my permission and my blessings!

“On the theory that the CFS rendered knowledge of the side subliminal, then even if subjects are above objective threshold, they should be at subjective threshold in indicating which side the image was on.”

I would be very happy to help you with this in further reviews if you decide to follow this method! If you do the latest I have written on this one is a direct replication manual for this method and I would be very happy to assist as a reviewer if you attempt it:

Tsikandilakis, M., Bali, P., Karlis, A., Mével, P. A., Madan, C., Derrfuss, J., & Milbank, A. (2023). Unbiased Individual Unconsciousness: Rationale, Replication and Developing Applications. *Current Research in Behavioral Sciences*, 100109.

I need to say that I had super-giga problems with APA images in the Appendix (they would not let me show them), and particularly in APA journals where not only they would not let me show them, but they also additionally asked for written and signed permission that I was eligible to use them. So please be prepared to receive “a bureaucratic attack” for every picture presented after page 31 in any most (all?) journals this fine work will be submitted to.

OK, I think, I will overwhelm you if I continue.

This is an excellent work. Truly well-done. I look forward to your responses and I will be very proud to see it *flourish and bloom* further.

Sincerely,

Myron Tsikandilakis