Response to reviewers…

Reviewer 1:

**Summary**

In this study, the authors tested whether non-daily smokers selectively attended to smoking imagery more than daily smokers. Using a well-established visual probe task, a high-powered design, and robust statistical tests, they found no significant difference between non-daily and daily smokers’ attentional bias. Moreover, their analysis revealed poor reliability of their task.

**Appraisal and major points**

I commend the authors—this study has all the makings of solid psychological science: preregistration, high-powered design, critical and cutting-edge statistical analyses, and clear and well-reasoned hypotheses. Despite the negative results, I believe this research has much to offer and, if anything, presents a powerful counterpoint to highly cited claims that currently exist in the literature. Nevertheless, there are few improvements I would like to see prior to publication. In no particular order:

**References**

Kappenman, E. S., MacNamara, A., & Proudfit, G. H. (2015). Electrocortical evidence for rapid allocation of attention to threat in the dot-probe task. *Social cognitive and affective neuroscience*, *10*(4), 577-583. https://doi.org/10.1093/scan/nsu098

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| Reviewer comment | Location | Our response |
| First, some of the details of the experimental procedure are a bit fuzzy to me. Were all pairings between images presented (i.e., fully factorial design: smoking-neutral; neutral-neutral; smoking-smoking) or did all presented pairs contains one smoking and one neutral cue? Obviously, the latter is the case of interest, but I was curious if these other, less informative, trials were present and, if so, how they processed (simply removed, used for exclusion but nothing else, etc.).  Also, which IAPS pictures were used as the neutral cues? I assume IAPS pictures labeled as neutral, but this would be important to confirm given the well-documented attentional bias effects towards threatening or negative imagery (e.g., Kappenman et al., 2015). Finally, it was not indicated how long the inter-trial fixation cross remained on the screen for. Many of these points could be addressed with the addition of a task figure to the manuscript, which I would recommend overall to readers who may be less familiar with the typical visual probe task. |  | Thank you for the prompt to make the materials clearer to readers.  We realised we relied on the open code to provide some of the processing details. We have added a new data processing sub-section to the results to explain how the neutral-neutral trials were excluded prior to analysis and the confirmatory analyses here focus on smoking/non-smoking trials.  In the original version of the manuscript, we included a disclosures section at the end which summarised the code/data links and provided a link to the Gorilla open materials page. We could create an anonymous version of the OSF link, but not to Gorilla, so we omitted the link from this version of the manuscript to comply with blinding. However, we have added the Gorilla open materials link back into the manuscript to demonstrate our response to this point.  To do – add IAPS images to OSF  We have also added a task diagram into the manuscript as Figure 1 to make the procedure easier to follow for readers who do not want to follow a link to Gorilla to preview the experiment – edit diagram to state dot and trial condition |
| Second, the authors test (rather well might I add) for the presence of a between-group effect: i.e., the existence of a difference in attentional bias between daily and non-daily smokers. At a more fundamental level however, the results presented in Figure 2 suggest to me that a basic attentional bias was not found within either group, beyond a difference in the group. While I am not an expert in attention bias and smoking research, this struck me as important, given that the authors cite many papers purportedly demonstrating this effect (albeit in a less-powered design, collapsing across daily and non-daily smokers). I suggest the authors interrogate the basic attentional bias effect, across groups and SOAs, which could be done in their current statistical framework by analyzing the significance of the intercept term in their ANOVA. Eyeballing the descriptive results reported in the paper, and Figure 2, it does not seem to me that any significant bias effect was found. If this analysis is done correctly—and with the degree of scrutiny the authors have committed to the rest of their analysis—I think this could be a strong challenge to what is apparently a quite widespread finding, given that, by the author’s own admission, their design is better powered than those in the past. |  |  |
| Lastly, I would suggest the authors briefly describe Laken’s (2018) small telescopes method for the readers. This is not yet a common statistical tool and I think it would bolster the impact of Figure 3 for readers to understand what is at stake with this analysis—namely that we can conclude, with high confidence, that the difference between groups is basically zero in both SOA conditions. In line with my previous comment, I would recommend running the same analysis on the basic attentional bias effect as well, using the same rationale. |  |  |
| On p. 4 the authors state that “This [the lack of reliability in many cognitive tasks] is not necessarily a problem for experimental research as the tasks are designed to emphasise differences between groups or conditions”. I am not totally convinced this is right, because, of course, for a test to have any resolution to predict group difference it must have some ability to discriminate between individuals. To take the extreme case, it is not possible for a test to be entirely unreliable at the individual level, but predict a group difference in the aggregate. I am nitpicking here to be sure, but perhaps consider rephrasing this section—after all, the importance of reliability in assessing group differences in the visual probe task is a key contribution of the present paper. |  |  |
| On the bottom of p. 6, the authors state “This procedure was repeated twice and presented in two blocks, creating 384 trials overall with 64 trials in each SOA and trial type condition.” What is the trial type condition? This is the only time this term is used. |  |  |

Reviewer 2

Two groups separated by daily smoking behavior were examined for attentional bias to some smoking related images.  My intial impression based on the abstract was that the authors were going to offer another slice of research for the lack of reliability pie (in attentional bias research), and show some data to support their hypotheses related to how these groups differ. But unforutnatly, there were two types of issues i could not see past.

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| Where is the logical connection between the data shown and the arguments made about these two groups (their differneces, why those exist, why readers might care).  Maybe the authors set out to resolve some mixed findings in the literature, but they ended up with another mixed finding.  Was there a specific hypothesis and rationale? Why were there seperate groups based on daily or non daily smoking?  What is gained by finding or not finding a group difference between daily and non daily smokers?  Too many times I was left with the impresssion that the authors wanted the collected data to speak for itself, without regaurd for the motivation to the research question. |  | Split introduction paragraph into two to add an explanation of why we were interested in AB in these two groups. Some background in the purpose section, but agree it was not expressed clearly on rereading. |
| Investigating attention bias is misguided with the typical approach: RT differences. It should only (and is rarely) be approached with methods that could gaurentee a higher chance of obtaining an effect with practical significnace (something with a SMD greater than .4, see Ferguson, C. J. (2009). An effect size primer: A guide for clinicians and researchers. *Professional Psychology: Research and Practice*, *40*(5), 532–538. <https://doi.org/10.1037/a0015808>).  RT differences are simply unable to produce a reliabile enough estimate to correlate with, well, much of anything (see the simons task and others here: Hedge, C., Powell, G., & Sumner, P. (2018). The reliability paradox: Why robust cognitive tasks do not produce reliable individual differences. *Behavior Research Methods*, *50*(3), 1166–1186. <https://doi.org/10.3758/s13428-017-0935-1>).  Given that the methods used here have such high limitations (although commonly used by researchers), their data cannot speak much to a research question. |  |  |