

When your heart isn't in it anymore: Cardiac correlates of task disengagement

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

In this manuscript, the authors investigate the relationship between cardiac measures and reported mental states during task performance. This is an interesting study that investigates the role of brain-heart interactions in cognitive performance. The manuscript is well written and clear.

The authors report that during a time window before the thought probes reporting mental state, mean interbeat interval (IBI) was higher for mind wandering or mind blanking than for on-task reports. IBI was also associated with task performance and pupil size. The amplitude of the neural responses to heartbeats, the heartbeat-evoked potentials (HEPs), was higher before mind blanking reports. Mutual information analysis did not reveal any changes in brain-heart communication across the different thought probes.

These observations appear to suggest that mind wandering and mind blanking are associated with periods of low arousal. However, I have some concerns regarding methodological details that might affect the findings and their interpretation and discussion. These are listed underneath.

1 – There is considerable evidence that errors induce task-related cardiac deceleration responses. Before mind wandering or mind blanking reports the participants committed larger number of errors, thus the error-related cardiac response might have led to an increase in mean IBI. In this case, changes in cardiac parameters will follow the increase in errors that will follow changes in state of mind. To better understand the origin of the study findings, it is important to investigate how task performance affects IBI and how this effect affects the cardiac measures used in this study.
For a review, see Di Gregorio et al 2024 Error-related cardiac deceleration: Functional interplay between error-related brain activity and autonomic nervous system in performance monitoring.

2 – HEP amplitude can be affected by changes in the dynamics of ongoing EEG signals expressed as slow drifts not locked with the ECG (see Box 4 of Azzalini et al 2019). Recently, two preprints have suggested important controls for this effect. State of mind might be associated with changes in the dynamics of the ongoing EEG, therefore, it is important that these controls are run to ensure that the differences in HEP amplitude reflect true responses locked with the R peak rather than non-specific differences. Calculation of surrogate HEPs locked to randomized R peaks might be informative to determine if the observed differences reflect neural responses to heartbeats or changes in baseline fluctuations.

Preprints:

Steinfath P, Herzog N, Fourcade A, Sander C, Nikulin V, Villringer A (2024) Validating genuine changes in Heartbeat Evoked Potentials using Pseudotrials and Surrogate Procedures. bioRxiv:2024.08.24.609348 Available at: <http://biorxiv.org/content/early/2024/08/26/2024.08.24.609348.abstract>.

Francesca Aprile, Marco Simões, Jorge Henriques, Paulo Carvalho, Miguel Castelo-Branco, Alejandra Sel, Maria J. Ribeiro (2024) The heartbeat-evoked potential in young and older adults during attention orienting. bioRxiv 2024.01.31.578137; doi: <https://doi.org/10.1101/2024.01.31.578137>

3 – It is unclear from the manuscript how and why the duration of the preprobe epochs were chosen. Also, these time periods appear to vary across the different physiological measures used. These intervals should be justified. How much the results might have changed if different choices were made?

Page 12 “Analyses involving attentional state reports were conducted using estimates derived from 10s preprobe epochs; all other analyses included summary statistics calculated from 30 s preprobe epochs.”

It is not clear, why these two time windows were used and in which analyses.

4 – In the methods section, page 5 – “Data from the final probe, which asked participants to rate their level of vigilance “over the past few trials” on a 4-point Likert scale (1 = “Extremely Sleepy”; 4 = “Extremely Alert”), are also reported.”

Was this the final probe for each probe trial, or the final probe of the experiment?

5 – For completeness, specify the formula for the calculation of the coefficient of variation.

6 – For the HEP analyses it is important to clean the EEG signal from the cardiac field artefact. The authors used the ICA method. However, it is not clear how many components were identified as cardiac noise and if these were identified in all participants. It would be important to identify and remove at least one cardiac component per participant to ensure that this source of noise was minimized.

7 – Blink artefacts in the pupil data were corrected through interpolation. However, the interpolation was only applied to the regions adjacent to the blinks and not to the blink itself. How were these data points interpolated? Usually, the whole blink period is substituted by interpolation taking into consideration the data points before and after the blink.

8 – Pupil measures from preprobe epochs were excluded based on a z-score threshold. How was this cutoff score determined and why? If these extreme values are related to physiological fluctuations, they should not be discarded as they reflect true physiology. For example, mind blanking might be associated with extreme values of these physiological markers and therefore excluding them could significantly change the results. How many epochs were discarded from each state of mind?

9 – For the calculation of cardiac parameters, outliers were also excluded based on z-score threshold. How was this cutoff determined and why?

10 – How many preprobe epochs were included for each analyses?

11 – In the discussion, page 17, the following sentence is not clear “Notably, mind-blanking states were accompanied by more pronounced heartbeat”

12 – Discussion, page 17 “Although phasic heartbeat fluctuations neither discriminated amongst attentional states nor covaried with vigilance level,” – what is meant by phasic heartbeat fluctuations? IBI fluctuations?

Reviewer #2

(Remarks to the Author)

Brief summary of the manuscript

The present study analyzed new behavioral indices, cardiac correlates, and brain-heart interactions not examined in Andrillon et al. (2021), using the same sample and task. The aim was to examine how cardiac activity relates to subjective, physiological, and behavioural indices of arousal and attentional fluctuations (on task, mind blanking and mind wandering) recorded during a sustained attention to response task (SART). Mind blanking was associated with poorer performance on the SART (lower sensitivity and increased bias), greater variability in response speed, higher interbeat intervals, and increased cortical processing of cardiac inputs. Mind wandering was related to lower sensitivity on the SART performance, greater variability in response speed and higher interbeat intervals. The increased interbeat intervals were also associated with lower vigilance and a poorer performance on the SART. In general, the results revealed that “task disengagement” is related to low arousal. Moreover, mind-blanking constitutes a low-arousal state distinct from other forms of task disengagement, such as mind-wandering. The results of this study are consistent with those of Andrillon et al. (2021) and provide complementary information about the physiological mechanisms underlying attentional lapses and vigilance levels.

Overall impression of the work

Overall, this study provides valuable insights into the relationship between cardiac activity and indices of arousal and attentional fluctuations during a sustained attention to response task. However, the scientific justification is weak, as well as the power of the study analyzing only 25 participants. Some concepts used, such as tonic and phasic cardiac activity, are unclear due to insufficient description. The data analysis appears scientifically sound, and I appreciate the transparency regarding the dataset and the analysis scripts. In summary, I think that the study is interesting; but the justification of methods and interpretation of results need to be improved.

Specific comments

Comment 1:

In the first paragraph, the authors introduce interoception and its relationship with cognitive variables such as perception, learning, and decision-making. However, as interoception is not directly measured in this study and is instead inferred indirectly, this topic seems better suited for the Discussion, where speculative interpretations are more appropriate. Since the study's primary objective is to examine cardiac correlates linked to subjective, physiological, and behavioral indices of attention and arousal, I recommend limiting the mention of interoception to the Discussion section. This adjustment would ensure that the Introduction remains focused and aligned with the study's specific aims and methods.

Comment 2:

In the second and third paragraphs of the Introduction, the authors propose an explanation of how afferent cardiac signals influence consciousness and behavior, suggesting that tonic cardiac activity provides contextual information about bodily and environmental states, while phasic activity delivers detailed updates about environmental dynamics. They even state that their objective includes assessing the contributions of phasic and tonic cardiac fluctuations to cognitive dynamics. However, at no point in the article do the authors clarify what they consider to be tonic and phasic cardiac activity. The terms “tonic” and “phasic” appear rarely in the remainder of the paper, and when they do, they refer ambiguously to “arousal” and the interpretation of heart rate variability results.

I assume the authors may be using these terms to describe the cardiac activity associated with task disengagement (interpreted as tonic) and task engagement (phasic). However, this interpretation is speculative given the limited contextualization and definition of these terms in the text. I would recommend either providing clear operational definitions for “tonic” and “phasic” cardiac activity in the Introduction and incorporating these concepts consistently throughout the paper or removing this objective if it is not directly assessed.

Comment 3:

A quick review of related literature reveals an article (cited below) that associates reduced cardiac reactivity with task disengagement. This study’s finding of increased inter-beat intervals during disengagement (indicative of decreased heart rate) aligns with previous research. However, the study of Behnke et al. (2021) is not cited in either the Introduction or the Discussion. Moreover, some studies about stress conceptualize the blunted cardiovascular reactivity to stress as reduced or absent task engagement in motivated performance (Hase et al., 2020). Including these citations would strengthen the manuscript by situating the study within the previous results on cardiac responses to task disengagement. I recommend incorporating these references to enhance the manuscript’s contextual grounding and highlight its contribution to this recent line of research.

Behnke, M., Hase, A., Kaczmarek, L.D. et al. Blunted cardiovascular reactivity may serve as an index of psychological task disengagement in the motivated performance situations. *Sci Rep* 11, 18083 (2021). <https://doi.org/10.1038/s41598-021-97670-0>

Hase, A., Aan Het Rot, M., de Miranda, A. R. & Freeman, P. Threat-related motivational disengagement: Integrating blunted cardiovascular reactivity to stress into the biopsychosocial model of challenge and threat. *Anxiety Stress Copin.* 33, 355–369. <https://doi.org/10.1080/10615806.2020.1755819>

Comment 4:

The Introduction would benefit from a more comprehensive review and explanation of prior research linking cardiac indices with attention and arousal. While the study aims to investigate these relationships, the rationale for the specific measures employed—such as heart evoked potential and mutual information—remains underdeveloped. It would be helpful if the authors could clarify what these indices contribute to the understanding of attention and arousal, as well as why they are particularly suited for this study. A more thorough justification for using these measures would provide readers with a clearer understanding of the study’s design choices and its potential contributions to the field.

Comment 5:

The last author of this paper was also the first author of a previous paper that used the same dataset, analyzing brain wave data collected from the same experimental task. This current study complements the previous work by examining cardiac activity to provide insights into how peripheral systems influence attention and vigilance metrics, also analyzed in the prior study. While the authors briefly mention the use of the same dataset and the complementary nature of this article, they do not clearly delineate what new insights this cardiac-focused analysis offers in relation to the prior publication. Strengthening the explanation of the present study’s unique contributions would better contextualize the motivation for publishing a second paper based on the same dataset and enhance readers’ understanding of the added value provided by this work.

Comment 6:

The study’s sample size is relatively small (25 participants), and no information is provided regarding the statistical power to justify this sample size. Additionally, the Discussion section does not address the limited generalizability of the findings due to the small sample. Please add the statistical power or address the limited generalizability of the findings in the Discussion.

Comment 7:

In the procedure section, the authors indicate that the attention task is intermittently interrupted by a STOP cue followed by 7 or 8 thought probes. While they describe the first and last thought probes in detail, the content and purpose of the intermediate thought probes are not clarified. I recommend providing a clear description of all the thought probes used in the task.

Comment 8:

The authors utilize response speed as an index in this study, whereas the previous article analyzed reaction times. Given that both measures assess similar constructs, I suggest that the authors clarify the rationale for choosing response speed over reaction time.

Comment 9:

In the behavioral results section, the authors state that their findings replicate those of the previous paper. However, given that the same dataset and measures are utilized in both studies, the term “replicate” may not be appropriately applied in this context. Instead, it might be more accurate to say that the findings are consistent with or confirm those of the earlier study. I recommend that the authors reconsider their use of the term “replicate”.

Comment 10:

It would be beneficial for the authors to include a figure or table presenting the means of the new behavioral indices (such as sensitivity and bias) introduced in this study in relation to attentional states.

Comment 11:

In the Discussion, I suggest again providing a clearer definition of tonic and phasic cardiac activity would enhance the reader's understanding of certain interpretations of the results.

Comment 12:

Please, revise the abstract after the corrections.

DECISION: Major revision

Reviewer #3

(Remarks to the Author)

Thank you for the opportunity to review this manuscript. The paper investigates the spontaneous fluctuations of attention and arousal during the Sustained Attention to Response Task (as self-reported by participants at pseudorandom probe-times) and their relationship to behavioural, cardiac and pupil indices. The results show that task disengagement (particularly mind-blanking) is related to reduced heart-rate, smaller pupil diameter, and reduced sensitivity. Mind-blanking and mind-wandering were found to have distinctive profiles.

Overall, I find the paper well-executed and well-written. I find the analyses appropriate and accurately interpreted. I believe the findings separating the profiles of mind-blanking and mind-wandering will be of interest to the readers interested in psychophysiology of attention. I have a number of comments related to the clarity and framing of the study, and to the inferences made.

1. Introduction: The rationale for the study (2nd paragraph of the intro) is somewhat unclear. The authors distinguish 'tonic' and 'phasic' cardiac dynamics and provide brief definitions, but those are quite vague, as is their relationship to the research question – please elaborate.
2. In this vein, please specify in the introduction what exactly is meant by 'tonic' and 'phasic' dynamics, including their signatures and timescales, and why they are expected to be related to attention. I would also recommend mentioning which cardiac parameters indexing tonic/phasic activity are being investigated here (next paragraph).
3. Introduction, 3rd paragraph: please mention the task during which the indices of arousal and attention are examined, perhaps with a brief description of the task.
4. Method: please clarify the exact trial numbers per block.
5. Data analysis: please clarify whether and how the pupil and ECG data were baseline-corrected.
6. Results: Have the post-response cardiac dynamics been assessed? Especially since orienting is mentioned in the discussion, I wonder whether potential error-related bradycardia could have spilled over to the subsequent trials? This is particularly relevant given the length of the preprobe epochs.
7. Discussion: the authors note that "the effects were observed after controlling for time-on-task, indicating that markers of parasympathetic outflow afford specific information (...) over and above generic effects pertaining to prolonged task performance". However, there may have been a physiological floor effect – the heart rate or pupil size cannot go down indefinitely or in perfect step with time-on-task – which was not captured by this control. Indeed, mind-wandering and -blanking alike were reported more frequently with time-on-task, so perhaps this outcome is not necessarily indicative of an effect 'over and above' – could the authors comment on that? Secondly, I encourage the authors to substantiate the point (if possible) that 'specific information' is afforded by the markers of parasympathetic outflow (what would this achieve functionally?), rather than them being correlates or drivers of task disengagement.
8. The authors close by noting that "neural processing and regulation of cardiac dynamics impact conscious experience..." (discussion, final paragraph). I would encourage caution here, given that cardiac dynamics observed here are a readout of the sympathetic/parasympathetic tone, itself already known to modulate arousal vs drowsiness. I am not convinced that one can isolate the impact of cardiac afferents specifically from the general autonomic pattern, and the earlier discussion is more cautious, so I encourage re-phrasing this sentence.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Thank you to the authors for the in-depth consideration of the reviewers' comments and suggestions. The authors addressed all my concerns, and the manuscript is now much improved. Nevertheless, upon reading the revised manuscript, I noticed some methodological aspects that require clarification.

1 - The mixed models used in most analyses provided a comparison between mind-wandering and on-task and mind-blanking and on-task but did not directly contrast mind-wandering and mind-blanking. These analyses and statistical results would be important to state that these two attentional states differ as claimed in the manuscript. This includes the analyses of the behavioral variables, heart rate, etc...

For example, in the discussion "Notably, mind-blanking states were accompanied by more pronounced reductions in heart-rate and response speed (RS_{μ}) than mind-wandering states, thus suggesting that mind-blanking is more likely to occur in the context of hypoarousal." and "Mind-wandering, by contrast, was characterised by subtler decreases in arousal that were associated with more stereotypic behaviour (increased response bias (c), unchanged response speed) and increased heart-rate variability (HRV; as quantified by IBI_{cv})."

– these statements are not supported by statistical comparisons directly contrasting mind-wandering and mind-blanking.

Importantly, the authors should specify their contrast coding choices for the categorical variables as explained in Laurel Brehm, Phillip M. Alday, Contrast coding choices in a decade of mixed models, *Journal of Memory and Language*, 2022 <https://www.sciencedirect.com/science/article/pii/S0749596X22000213>

2 - Maybe I am not quite understanding - the authors used the generalised additive mixed-effects model framework thereby controlling for performance fluctuations over time including effects of time-on-task. So, when the authors study the effect of the independent variable time-on-task (probe number) is this variable added to the model with the factor smooths? Wouldn't both capture the same variance?

3 - In the section "Cardiac measures afford complementary information about arousal, attentional, and behavioural states", in the reporting of the results from the mixed models, it is not clear which were the dependent and which were the independent variables? Please clarify.

4 - In the discussion: "Independent of attentional state, greater HRV predicted increased pupil dilation, response bias, response speed, and false alarm rate, suggesting transient fluctuations in heartbeat dynamics afford unique information pertaining to what appear to be impulsive patterns of behaviour." Independent of attentional state? Are these analyses run in independent models? Was attentional state and interactions included in the models? Which statistical analysis supports this statement?

5 - In the section, "Error-related cardiac deceleration does not account for differences across attentional states", the sentence: "Interestingly, IBIs tended to be longer in sequences spanning a correct rejection ($F(1) = 23.95, p < .001$), irrespective of the subsequent attentional state report.", suggests the lack of an interaction between the effect of IBI order and attentional state report but this statistical result is not presented. Please complete the statistical reporting including all the effects and interactions included in the model.

6 - Analyses of HEP and MI were not controlled for time on task? This would be important as mind-wandering and blanking increase with time-on-task.

7 - On the discussion of the HEP results: "Late modulation of heartbeat-evoked responses further indicates that mind-wandering is associated with amplified cortical processing of interoceptive signals." – the differences in amplitude of the HEP can only be interpreted as changes in the neural response. If we interpret the amplitude as a difference from zero it even suggests a decrease in absolute amplitude in the mind-wandering state and not otherwise, but given the overlap between different neural components possibly composing the HEP, this interpretation needs to be made with caution.

8 - "Lower heart-rate predicted decreased vigilance and pupil size, in addition to slower, less-biased responses; increased heart-rate variability predicted more impulsive behaviour and pupil dilation."

I suggest avoiding describing the results using the verb predict as for some variables the association is not obviously directional: for example, pupil size and cardiac measures are associated but either might predict the other.

9 – EEG preprocessing: Although possibly OK, running ICA twice after removing components is not advisable. Just a warning to avoid in the future. See:

https://eeglab.org/tutorials/06_RejectArtifacts/RunICA.html

Reviewer #3

(Remarks to the Author)

The authors have responded to all my comments and concerns and made revisions accordingly. I am happy to recommend the revised manuscript for publication.

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors addressed all my concerns, the manuscript is now much improved. I have no further comments.

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Reviewer #1

In this manuscript, the authors investigate the relationship between cardiac measures and reported mental states during task performance. This is an interesting study that investigates the role of brain-heart interactions in cognitive performance. The manuscript is well written and clear.

[Thank you for this positive assessment of our work.](#)

The authors report that during a time window before the thought probes reporting mental state, mean interbeat interval (IBI) was higher for mind wandering or mind blanking than for on-task reports. IBI was also associated with task performance and pupil size. The amplitude of the neural responses to heartbeats, the heartbeat-evoked potentials (HEPs), was higher before mind blanking reports. Mutual information analysis did not reveal any changes in brain-heart communication across the different thought probes.

These observations appear to suggest that mind wandering and mind blanking are associated with periods of low arousal. However, I have some concerns regarding

methodological details that might affect the findings and their interpretation and discussion. These are listed underneath.

We appreciate these concerns and have endeavoured to address them as comprehensively as possible in our revision.

1. There is considerable evidence that errors induce task-related cardiac deceleration responses. Before mind wandering or mind blanking reports the participants committed larger number of errors, thus the error-related cardiac response might have led to an increase in mean IBI. In this case, changes in cardiac parameters will follow the increase in errors that will follow changes in state of mind. To better understand the origin of the study findings, it is important to investigate how task performance affects IBI and how this effect affects the cardiac measures used in this study.

For a review, see Di Gregorio et al 2024 Error-related cardiac deceleration: Functional interplay between error-related brain activity and autonomic nervous system in performance monitoring.

This is an excellent observation. Indeed, our preliminary analysis of the cardiac deceleration response yielded no significant differences between attentional states; it was an oversight not to mention this (lack of) effect in the original manuscript. We have now included a new section in the Results reporting this analysis (Section “Error-related cardiac deceleration does not account for differences across attentional states”; pp. 20-21).

2. HEP amplitude can be affected by changes in the dynamics of ongoing EEG signals expressed as slow drifts not locked with the ECG (see Box 4 of Azzalini et al 2019). Recently, two preprints have suggested important controls for this effect. State of mind might be associated with changes in the dynamics of the ongoing EEG, therefore, it is important that these controls are run to ensure that the differences in HEP amplitude reflect true responses locked with the R peak rather than non-specific differences. Calculation of surrogate HEPs locked to randomized R peaks might be informative to determine if the observed differences reflect neural responses to heartbeats or changes in baseline fluctuations.

Preprints:

Steinfath P, Herzog N, Fourcade A, Sander C, Nikulin V, Villringer A (2024) Validating genuine changes in Heartbeat Evoked Potentials using Pseudotrials and Surrogate Procedures. bioRxiv:2024.08.24.609348 Available at: <http://biorxiv.org/content/early/2024/08/26/2024.08.24.609348.abstract>.

Francesca Aprile, Marco Simões, Jorge Henriques, Paulo Carvalho, Miguel Castelo-Branco, Alejandra Sel, Maria J. Ribeiro (2024) The heartbeat-evoked potential in young and older adults during attention orienting. bioRxiv 2024.01.31.578137; doi: <https://doi.org/10.1101/2024.01.31.578137>

Thank you for drawing this work to our attention; we appreciate the importance of differentiating cardiac-specific effects from more generic changes in the EEG. We have now implemented a surrogate R-peak analysis following the procedures reported in Steinfath et al (2024). The details of this analysis are as follows:

P. 15: As a further precaution against the possibility of spurious results marring our HEP analysis, t-values from significant clusters resolved using TFCE were compared to those generated by replicating the mass univariate analysis on surrogate R-peaks^{73,148}. We created surrogate R-peaks by randomly permuting each IBI time-series within the 40 s period preceding each set of thought probes and time-locking our HEP analysis to the latencies implied by the reordered IBIs. This procedure was repeated 100 times to build a null distribution of surrogate t-values that reflected differences in EEG fluctuations that were not systematically related to the heartbeat (since the temporal dependence between EEG responses and cardiac systole had been broken)¹⁴⁹. Observed t-values that failed to exceed the permutation threshold defining a Monte Carlo p-value < .05 (two-tailed) were masked as non-significant.

3. It is unclear from the manuscript how and why the duration of the preprobe epochs were chosen. Also, these time periods appear to vary across the different physiological measures used. These intervals should be justified. How much the results might have changed if different choices were made?

Page 12 “Analyses involving attentional state reports were conducted using estimates derived from 10s preprobe epochs; all other analyses included summary statistics calculated from 30 s preprobe epochs.”

It is not clear, why these two time windows were used and in which analyses.

We appreciate the analysis of differing time-intervals might be a cause for confusion; hence, we have now streamlined our analysis by focusing exclusively on 10 s preprobe epochs. Reassuringly, comparing the findings of our updated analysis to those presented in the original submission reveals a very similar pattern of results as was obtained from analysis of the 30 s epoch durations. Since all data and code are openly available, the interested reader may explore the effect of varying epoch durations at their discretion.

4. In the methods section, page 5 – “Data from the final probe, which asked participants to rate their level of vigilance “over the past few trials” on a 4-point Likert scale (1 = “Extremely Sleepy”; 4 = “Extremely Alert”), are also reported.”

Was this the final probe for each probe trial, or the final probe of the experiment?

We were referring to the final probe within each set of 60 probes. This ambiguity has now been clarified as follows:

*P. 6: Data from the final probe **of each set**, which asked participants to rate their level of vigilance “over the past few trials” on a 4-point Likert scale (1 = “Extremely Sleepy”; 4 = “Extremely Alert”), are also reported.*

5. For completeness, specify the formula for the calculation of the coefficient of variation.

We now include the coefficient of variation formula in the relevant subsections of the Methods:

P. 12: [...] the coefficient of variation (RSc_v) for each preprobe epoch obtained by dividing the standard deviation of the interpolated series ($RS_{\square}$) by the corresponding mean ($RS_{\square}$):

$$RSc_v = \frac{RS\sigma}{RS\mu}.$$

[...] Analogous to the reaction speed analysis, mean IBI (IBI_{μ}) and coefficient of variation ($IBIc_v$) estimates were calculated for each preprobe epoch. ***IBIc_v was natural log-transformed as follows:***

$$IBIc_v = \ln\left(\frac{IBI\sigma}{IBI\mu}\right).$$

6. For the HEP analyses it is important to clean the EEG signal from the cardiac field artefact. The authors used the ICA method. However, it is not clear how many components were identified as cardiac noise and if these were identified in all participants. It would be important to identify and remove at least one cardiac component per participant to ensure that this source of noise was minimized.

In our original analysis we allowed for automated detection of ICs classed as cardiac in nature with >90% confidence by EEGLab's ICLabel routine. This approach was deemed likely to promote the removal of strong cardiac-related signals from the EEG while minimising the distortion of neural components. However, dominant cardiac-related artifacts were not identified by ICLabel in every dataset; hence, we concur with the Reviewer that it would be prudent to take additional steps to mitigate this potential source of artifact. We thus took the opportunity to revise our preprocessing pipeline such that it now includes an additional round of ICA designed to specifically isolate major CFA-related components within epochs time-locked to the R-wave. These components were subtracted from each dataset prior to conducting brain-heart coupling analyses. The procedure was as follows:

P. 10: Data were then subjected to a second round of ICA specifically designed to mitigate the impact of the cardiac field artefact. EEG records were epoched [-200, 200] ms relative to each R-peak and decomposed via ICA. Component and ECG time-series were then low-pass filtered below 25 Hz using a 2-pass, 2nd-order Butterworth filter and Hilbert-transformed to obtain instantaneous phase estimates. Coherence of each component with the ECG signal was calculated as the mean difference between each pair of component-ECG phase angles^{119,120}. Components scoring > 3 S.D. above the subject-level mean coherence were marked as cardiac in nature and subtracted from the full EEG dataset. This procedure was repeated until a maximum of 3 components were excluded, or until no remaining coherence values exceeded the rejection threshold (which was recalculated on each iteration; median number of rejected components = 2; range = [0, 3]). For similar approaches, see Banellis and Cruse¹²¹ and Buot and colleagues¹²².

7. Blink artefacts in the pupil data were corrected through interpolation. However, the interpolation was only applied to the regions adjacent to the blinks and not to the blink itself. How were these data points interpolated? Usually, the whole blink period is substituted by interpolation taking into consideration the data points before and after the blink.

This indeed is what we did; the interpolation spanning the rejected period was calculated from the average pupil size in the [-200, -100] ms prior to the blink and the [100, 200] ms

following the blink. We have made our description of this procedure clearer in the Methods section:

*P. 10: Blink timings (automatically identified by the EyeLink acquisition software) were corrected via linear interpolation **over the blink interval. The start and end points of the interpolated segment were calculated by taking the average pupil size spanning [-200, -100] ms before and [100, 200] ms after the blink period, respectively.***

8. Pupil measures from preprobe epochs were excluded based on a z-score threshold. How was this cutoff score determined and why? If these extreme values are related to physiological fluctuations, they should not be discarded as they reflect true physiology. For example, mind blanking might be associated with extreme values of these physiological markers and therefore excluding them could significantly change the results. How many epochs were discarded from each state of mind?

Given these concerns we have opted not to apply this threshold to the pupil data in the revised analysis. However, we did exclude epochs missing >50% of pupil data from our analysis, on the basis that these epochs were too noisy to reliably estimate pupil size.

9. For the calculation of cardiac parameters, outliers were also excluded based on z-score threshold. How was this cutoff determined and why?

Although the annotation of R-peaks within the ECG trace was manually reviewed and corrected where necessary, periods of signal contamination resulted in transient distortions that prevent the precise localisation of R-peak events. Such missing R-peaks result in unrealistically long IBIs, where more than one IBI has been merged. These few extreme IBIs need to be removed from the time series as they can profoundly bias estimates of HRV. Since we are able to reliably resolve thousands of IBIs on the subject level, and these IBIs are approximately Gaussian-distributed, individualised z-score thresholds afford an efficient way to identify and exclude such extreme data points. It is important to reiterate that these few excluded IBIs are not simply outliers belonging to the tails of the IBI distribution, but artefacts caused by rare moments of substantial signal distortion. They are as such relatively easy to distinguish using an extreme (i.e., conservative) threshold such as $|z.u.| > 5$, which we applied in our study. Note that we apply the threshold to both sides of the distribution to guard against implausibly short IBIs that may occasionally be registered due to an R-peak event being marked twice in (very) close succession – such rare artefacts are easy to miss when manually reviewing the data.

*P. 12: IBIs were z-transformed on the subject-level, and scores exceeding ± 5 z.u. excluded from analysis **(on the basis such extreme outliers were likely artifacts due to missing or spurious R-peak annotations; median number of rejected IBIs = 2; range = [0, 30]).***

10. How many preprobe epochs were included for each analyses?

Each analysis was performed on the 60 preprobe epochs recorded from each participant unless an epoch was discarded due to insufficient data for one of the variables included in

the model (e.g., few behavioural responses, missing pupil size data). We make the number of excluded preprobe epochs explicit as follows:

*P. 11: A minimum of 5 **valid** responses was required to **estimate RS** for a given epoch (median number of rejected epochs = 3; range = [0, 39]).*

*P. 12: **Preprobe** epochs containing < 5 s of pupil size **data** were excluded from analysis (median number of rejected epochs = 0; range = [0, 24]).*

*P. 12: **R-peaks followed by IBI durations < 600 ms were discarded, and a minimum of 5 R-peaks was required to estimate the HEP for each individual epoch (these criteria resulted in the exclusion of 8 preprobe epochs from one participant and 1 preprobe epoch from another).***

11. In the discussion, page 17, the following sentence is not clear “Notably, mind-blanking states were accompanied by more pronounced heartbeat”

Apologies for the confusion, the sentence now reads as follows:

*P. 27: Notably, mind-blanking states were accompanied by more pronounced **reductions in heart-rate** and response **speed (RS)** than mind-wandering states, thus suggesting that mind-blanking is more likely to occur in the context of hypoarousal.*

12. Discussion, page 17 “Although phasic heartbeat fluctuations neither discriminated amongst attentional states nor covaried with vigilance level,” – what is meant by phasic heartbeat fluctuations? IBI fluctuations?

Yes – however, in light of the comments made by the other two Reviewers, we now use clearer language pertaining to the specific cardiac parameters included in our analysis and refrain from discussing tonic and phasic fluctuations.

Reviewer #2

The present study analyzed new behavioral indices, cardiac correlates, and brain-heart interactions not examined in Andrillon et al. (2021), using the same sample and task. The aim was to examine how cardiac activity relates to subjective, physiological, and behavioural indices of arousal and attentional fluctuations (on task, mind blanking and mind wandering) recorded during a sustained attention to response task (SART). Mind blanking was associated with poorer performance on the SART (lower sensitivity and increased bias), greater variability in response speed, higher interbeat intervals, and increased cortical processing of cardiac inputs. Mind wandering was related to lower sensitivity on the SART performance, greater variability in response speed and higher interbeat intervals. The increased interbeat intervals were also associated with lower vigilance and a poorer performance on the SART. In general, the results revealed that “task disengagement” is related to low arousal. Moreover, mind-blanking constitutes a low-arousal state distinct from other forms of task disengagement, such as mind-wandering. The results of this study are

consistent with those of Andrillon et al. (2021) and provide complementary information about the physiological mechanisms underlying attentional lapses and vigilance levels.

Overall, this study provides valuable insights into the relationship between cardiac activity and indices of arousal and attentional fluctuations during a sustained attention to response task. However, the scientific justification is weak, as well as the power of the study analyzing only 25 participants. Some concepts used, such as tonic and phasic cardiac activity, are unclear due to insufficient description. The data analysis appears scientifically sound, and I appreciate the transparency regarding the dataset and the analysis scripts. In summary, I think that the study is interesting; but the justification of methods and interpretation of results need to be improved.

We thank the reviewer for acknowledging the merits of our original study and welcome the opportunity to clarify the framing and interpretation of our analysis. In response to concerns about sample size, we have collected and analysed a substantial amount of new data, resulting in a 3-fold increase in the overall sample size. This also means that $\frac{2}{3}$ of the data we analysed is novel and replicates our previous protocol. We are confident that the findings reported in the revised manuscript are robust.

1. In the first paragraph, the authors introduce interoception and its relationship with cognitive variables such as perception, learning, and decision-making. However, as interoception is not directly measured in this study and is instead inferred indirectly, this topic seems better suited for the Discussion, where speculative interpretations are more appropriate. Since the study's primary objective is to examine cardiac correlates linked to subjective, physiological, and behavioral indices of attention and arousal, I recommend limiting the mention of interoception to the Discussion section. This adjustment would ensure that the Introduction remains focused and aligned with the study's specific aims and methods.

It is important to note that interoceptive processing is not exclusively related to conscious detection or recognition of internal signals, just as visual processing does not necessarily entail conscious perception. It is uncontroversial in the field (as evidenced by the reviews of interoception and brain-body interaction we cite) that the brain is constantly monitoring and adjusting physiological parameters, and that much of this processing goes on outside of conscious awareness (and indeed, persist in the complete absence of consciousness; e.g., during deep sleep and anaesthetised states).

We maintain that our study is directly concerned with interoceptive processing insofar as it investigates cardiac dynamics that are regulated by the brain in response to interoceptive information – brain-heart coupling is intrinsically interoceptive in nature. We have modified our introduction to clarify that we are concerned with a broader understanding of interoception as follows:

*P. 3: **Neural monitoring of internal bodily states (interoception)** is essential for homeostasis, and conditions the way we perceive and act in the world^{1–5}.*

2. In the second and third paragraphs of the Introduction, the authors propose an explanation of how afferent cardiac signals influence consciousness and behavior, suggesting that tonic cardiac activity provides contextual information about bodily and environmental states, while phasic activity delivers detailed updates about environmental dynamics. They even state that their objective includes assessing the contributions of phasic and tonic cardiac fluctuations to cognitive dynamics. However, at no point in the article do the authors clarify what they consider to be tonic and phasic cardiac activity. The terms “tonic” and “phasic” appear rarely in the remainder of the paper, and when they do, they refer ambiguously to “arousal” and the interpretation of heart rate variability results.

I assume the authors may be using these terms to describe the cardiac activity associated with task disengagement (interpreted as tonic) and task engagement (phasic). However, this interpretation is speculative given the limited contextualization and definition of these terms in the text. I would recommend either providing clear operational definitions for “tonic” and “phasic” cardiac activity in the Introduction and incorporating these concepts consistently throughout the paper or removing this objective if it is not directly assessed.

We appreciate that these important terms are deployed in different ways in the literature and would therefore benefit from more explicit definition and discussion. In light of the Reviewer's feedback, we have opted to avoid these terms, and focus our Introduction more specifically on the current state of the art in the cognitive neuroscientific literature on brain-body interactions, attention, and spontaneous cognitive dynamics.

3. A quick review of related literature reveals an article (cited below) that associates reduced cardiac reactivity with task disengagement. This study's finding of increased inter-beat intervals during disengagement (indicative of decreased heart rate) aligns with previous research. However, the study of Behnke et al. (2021) is not cited in either the Introduction or the Discussion. Moreover, some studies about stress conceptualize the blunted cardiovascular reactivity to stress as reduced or absent task engagement in motivated performance (Hase et al., 2020). Including these citations would strengthen the manuscript by situating the study within the previous results on cardiac responses to task disengagement. I recommend incorporating these references to enhance the manuscript's contextual grounding and highlight its contribution to this recent line of research.

Behnke, M., Hase, A., Kaczmarek, L.D. et al. Blunted cardiovascular reactivity may serve as an index of psychological task disengagement in the motivated performance situations. *Sci Rep* 11, 18083 (2021). <https://doi.org/10.1038/s41598-021-97670-0>

Hase, A., Aan Het Rot, M., de Miranda, A. R. & Freeman, P. Threat-related motivational disengagement: Integrating blunted cardiovascular reactivity to stress into the biopsychosocial model of challenge and threat. *Anxiety Stress Copin.* 33, 355–369. <https://doi.org/10.1080/10615806.2020.1755819>

Thank you for drawing these papers to our attention; although they deal with different concepts of task disengagement than mind-wandering and mind-blanking, they are clearly relevant and complementary to our own study. They have now been incorporated into our revised Introduction:

P. 4: Beyond the mind-wandering and mind-blanking literature, subjective disengagement from challenging tasks has been associated with blunted increases in heart-rate relative to resting-state^{95,96}, indicating that cardiac dynamics afford information about one's willingness to invest effort in task performance.

4. The Introduction would benefit from a more comprehensive review and explanation of prior research linking cardiac indices with attention and arousal. While the study aims to investigate these relationships, the rationale for the specific measures employed—such as heart evoked potential and mutual information—remains underdeveloped. It would be helpful if the authors could clarify what these indices contribute to the understanding of attention and arousal, as well as why they are particularly suited for this study. A more thorough justification for using these measures would provide readers with a clearer understanding of the study's design choices and its potential contributions to the field.

We accept that our attempt to provide a succinct Introduction resulted in the omission of information that would be useful for orienting the reader. We have therefore thoroughly revised this section to provide more context on the connection between cardiovascular activity, arousal, and attention, and our rationale for focusing on the time-domain HEP and frequency-domain MI estimators as complementary indicators of brain-heart interaction.

5. The last author of this paper was also the first author of a previous paper that used the same dataset, analyzing brain wave data collected from the same experimental task. This current study complements the previous work by examining cardiac activity to provide insights into how peripheral systems influence attention and vigilance metrics, also analyzed in the prior study. While the authors briefly mention the use of the same dataset and the complementary nature of this article, they do not clearly delineate what new insights this cardiac-focused analysis offers in relation to the prior publication. Strengthening the explanation of the present study's unique contributions would better contextualize the motivation for publishing a second paper based on the same dataset and enhance readers' understanding of the added value provided by this work.

As is hopefully now clear in the revised Introduction, we see the current work as contributing primarily to the burgeoning field of brain-body interactions, focusing in particular on the way in which interoceptive states may influence spontaneous cognition (understood in terms of task-focused, mind-wandering, and mind-blanking states). This topic is completely independent of the questions addressed by Andrillon and colleagues' original study, which was primarily concerned with characterising the neural and behavioural signatures of mind-blanking and mind wandering and their relation to slow-wave dynamics. In this previous study, there was no analysis of cardiac activity, which is central to the present study. Please also note the relation between this manuscript and the previous publication is now very much weakened given the new data that has now been included (see next point).

6. The study's sample size is relatively small (25 participants), and no information is provided regarding the statistical power to justify this sample size. Additionally, the Discussion section does not address the limited generalizability of the findings due to the small sample. Please add the statistical power or address the limited generalizability of the findings in the Discussion.

We have addressed this concern by collecting and analysing data from an additional 40 participants, reaching a total sample size of 65 individuals.

7. In the procedure section, the authors indicate that the attention task is intermittently interrupted by a STOP cue followed by 7 or 8 thought probes. While they describe the first and last thought probes in detail, the content and purpose of the intermediate thought probes are not clarified. I recommend providing a clear description of all the thought probes used in the task.

These probes were not described in the manuscript because they were not relevant to the aims of the present study. We are providing now a full description of these probes in the Methods section. We clarify this point:

Pp. 6-7: Data from the final probe of each set, which asked participants to rate their level of vigilance “over the past few trials” on a 4-point Likert scale (1 = “Extremely Sleepy”; 4 = “Extremely Alert”), are also reported. In addition, participants were also asked to answer the following questions, which were not analysed here: (1) “Were you looking at the screen?” (response: yes / no); (2) “How aware were you of your focus?” (response: from 1, I was fully aware, to 4, I was not aware at all); (3) “Was your state of mind intentional?” (response: from 1, entirely intentional, to 4, entirely unintentional); (6) “How engaging were your thoughts?” (response: from 1, not engaging, to 4, very engaging); (7) “How well do you think you have been performing?” (response: from 1, not good, to 4, very good).

8. The authors utilize response speed as an index in this study, whereas the previous article analyzed reaction times. Given that both measures assess similar constructs, I suggest that the authors clarify the rationale for choosing response speed over reaction time.

Response speed was favoured on account of rendering a more normally-distributed metric than its inverse, response time – which often requires some form of nonlinear (e.g., log or square-root) transformation. Moreover, response speed affords an arguably more intuitive interpretation than reaction time, insofar as speed increases correspond to faster responses. The preprocessed data and script underpinning this analysis are available on GitHub; it would be trivial to refit the models to reaction time estimates if one were so inclined.

9. In the behavioral results section, the authors state that their findings replicate those of the previous paper. However, given that the same dataset and measures are utilized in both studies, the term “replicate” may not be appropriately applied in this context. Instead, it might be more accurate to say that the findings are consistent with or confirm those of the earlier study. I recommend that the authors reconsider their use of the term “replicate”.

We understand the Reviewer’s comment. The term “replicate” is a bit ambiguous here. However, since we added new data (40 new participants), this concern no longer applies. As a result, we have removed this term and substantially revised this section to include results from the new dataset.

10. It would be beneficial for the authors to include a figure or table presenting the means of the new behavioral indices (such as sensitivity and bias) introduced in this study in relation to attentional states.

Thank you for this suggestion; we are happy to include this additional information in the form of a new table, which we split according to the origin of the data (original dataset: Melbourne; new dataset: Paris):

<i>Site</i>	<i>Melbourne</i>			<i>Paris</i>		
<i>State</i>	<i>ON</i>	<i>MW</i>	<i>MB</i>	<i>ON</i>	<i>MW</i>	<i>MB</i>
<i>d'</i>	1.67 (0.66)	1.44 (0.76)	1.36 (0.84)	1.52 (0.75)	1.15 (0.82)	0.99 (0.88)
<i>c</i>	0.52 (0.34)	0.60 (0.36)	0.52 (0.34)	0.51 (0.34)	0.57 (0.40)	0.48 (0.47)
<i>RS</i> □	1.86 (0.31)	1.93 (0.33)	1.82 (0.30)	2.27 (0.35)	2.29 (0.35)	2.20 (0.36)
<i>RSc_v</i>	0.17 (0.08)	0.18 (0.09)	0.20 (0.09)	0.17 (0.07)	0.18 (0.08)	0.20 (0.08)

Table 1. Summary statistics for behavioural measures calculated under each attentional state for both datasets.

Mean (standard deviation) for sensitivity (*d'*), bias (*c*), response speed (*RS*□) and coefficient of variation (*RSc_v*) measures calculated within each attentional state response category for both the Melbourne and Paris datasets. ON = on-task; MW = mind-wandering; MB = mind-blanking.

11. In the Discussion, I suggest again providing a clearer definition of tonic and phasic cardiac activity would enhance the reader's understanding of certain interpretations of the results.

In light of your comments – and those from Reviewer 3 – we have opted to avoid this ambiguous language. We now focus our Discussion more specifically on the parameters that were included in the analysis.

12. Please, revise the abstract after the corrections.

Done.

Reviewer #3

Thank you for the opportunity to review this manuscript. The paper investigates the spontaneous fluctuations of attention and arousal during the Sustained Attention to Response Task (as self-reported by participants at pseudorandom probe-times) and their relationship to behavioural, cardiac and pupil indices. The results show that task disengagement (particularly mind-blanking) is related to reduced heart-rate, smaller pupil

diameter, and reduced sensitivity. Mind-blanking and mind-wandering were found to have distinctive profiles.

Overall, I find the paper well-executed and well-written. I find the analyses appropriate and accurately interpreted. I believe the findings separating the profiles of mind-blanking and mind-wandering will be of interest to the readers interested in psychophysiology of attention. I have a number of comments related to the clarity and framing of the study, and to the inferences made.

Thank you for these kind remarks. We have endeavoured to improve the framing of the study and discussion of our findings in response to the points that follow.

1. Introduction: The rationale for the study (2nd paragraph of the intro) is somewhat unclear. The authors distinguish 'tonic' and 'phasic' cardiac dynamics and provide brief definitions, but those are quite vague, as is their relationship to the research question – please elaborate.

We agree with this and have substantially revised the entire Introduction section of the manuscript to better contextualise the study.

2. In this vein, please specify in the introduction what exactly is meant by 'tonic' and 'phasic' dynamics, including their signatures and timescales, and why they are expected to be related to attention. I would also recommend mentioning which cardiac parameters indexing tonic/phasic activity are being investigated here (next paragraph).

In light of this and similar comments from Reviewer 2, we have refrained from discussing the tonic / phasic distinction, focusing instead on the cardiac parameters relevant to the analysis.

3. Introduction, 3rd paragraph: please mention the task during which the indices of arousal and attention are examined, perhaps with a brief description of the task.

We now mention the SART in the final paragraph of the revised Introduction:

*P. 5: [...] we sought to identify the cardiac signatures of **task (dis)engagement** during the performance of a **Go/NoGo paradigm known as the sustained attention to response task (SART^{103,104})**. We evaluated how cardiac parameters varied as a function of participants' focus of attention (on-task, mind-wandering, mind-blanking) while performing the SART, and how these profiles compared with established indices of arousal and task (dis)engagement (i.e., subjective alertness/fatigue, pupil size, behavioural performance).*

4. Method: please clarify the exact trial numbers per block.

The precise number of trials varied, explaining the differences in block duration. We have clarified this as follows:

*P. 7: Blocks **comprised between 459 and 616 trials (median = 540)**, lasted approximately 12–15 min, and were separated by self-paced breaks (total duration: $M = 95.1$ min, $S.D. = 10.4$).*

5. Data analysis: please clarify whether and how the pupil and ECG data were baseline-corrected.

Our paradigm did not include baseline periods, since we were interested in the way subjective reports and psychophysiological parameters evolved over sustained periods of time. Instead, data collected over the entire session were z-score normalised on the participant-level such that differences across states could be meaningfully interpreted in accordance with the amount of variability expressed within each individual.

Please also note that we opted not to ‘baseline-correct’ heartbeat-evoked potentials, since doing so risks distorting or confounding estimates with fluctuations deriving from the preceding cardiac cycle. We now make this point explicit in the Methods:

*P. 12: **Epoches data were not ‘baseline-corrected’, since this procedure may contaminate HEP estimates with fluctuations deriving from the preceding cardiac cycle***^{1,68}.

6. Results: Have the post-response cardiac dynamics been assessed? Especially since orienting is mentioned in the discussion, I wonder whether potential error-related bradycardia could have spilled over to the subsequent trials? This is particularly relevant given the length of the preprobe epochs.

This is an excellent point; we have added a new analysis of cardiac dynamics centred on NoGo trials to address the possibility that erroneous responding may have impacted mean IBI duration and variation (see Section “Error-related cardiac deceleration does not account for differences across attentional states”; pp. 20-21).

7. Discussion: the authors note that “the effects were observed after controlling for time-on-task, indicating that markers of parasympathetic outflow afford specific information (...) over and above generic effects pertaining to prolonged task performance”. However, there may have been a physiological floor effect – the heart rate or pupil size cannot go down indefinitely or in perfect step with time-on-task – which was not captured by this control. Indeed, mind-wandering and -blanking alike were reported more frequently with time-on-task, so perhaps this outcome is not necessarily indicative of an effect ‘over and above’ – could the authors comment on that? Secondly, I encourage the authors to substantiate the point (if possible) that ‘specific information’ is afforded by the markers of parasympathetic outflow (what would this achieve functionally?), rather than them being correlates or drivers of task disengagement.

While it is true that psychophysiological measures are bounded, we do not think this would undermine our modelling of time-on-task. Yet, we understand the Reviewer’s concern that we cannot fully account for the time-on-task effect and we now make it clear that this is a statistical control.

We included factor smooths in our generalised additive mixed models, which act like time-dependent random intercepts capturing each individual's expected response across the course of the experimental session. Had any psychophysiological response variable declined to its floor level, the factor smooth component of the model would have captured this, and there would be no further fluctuation in the response variable to be captured by the fixed effects structure. In other words, systematic variance attributed to some independent variable (predictor) must have been expressed over and above the modelled change in the response variable for each individual over time – moreover, this variance must have been sufficiently consistent across subjects to render a significant fixed effect estimate. We now highlight our use of factor smooths as a statistical control for time-on-task in the Discussion:

*P. 28: Notably, the effects **we report here** were observed after **statistically** controlling for time-on-task (i.e., by specifying factor-smooths on probe number for each individual participant) [...]*

Regarding the meaning of “specific information”, we now explain that we meant that the control for time-on-task effect suggests that the “markers of parasympathetic outflow” carry information related to cardiac activity:

*Pp. 28-29: [...] markers of parasympathetic outflow afford specific information (**related to cardiac activity**) about the transition from on-task through to mind-blanking states over and above generic effects pertaining to prolonged task performance (e.g., motivational state, mental fatigue, drowsiness, etc.) ^{171–175}.*

8. The authors close by noting that “neural processing and regulation of cardiac dynamics impact conscious experience...” (discussion, final paragraph). I would encourage caution here, given that cardiac dynamics observed here are a readout of the sympathetic/parasympathetic tone, itself already known to modulate arousal vs drowsiness. I am not convinced that one can isolate the impact of cardiac afferents specifically from the general autonomic pattern, and the earlier discussion is more cautious, so I encourage re-phrasing this sentence.

We appreciate this concern, and have reformulated the concluding paragraph as follows:

P. 31: Together, these findings highlight how the neural processing and regulation of cardiac activity varies as a function of attentional state, shedding light on the way brain-body interactions shape cognitive and behavioural dynamics subtending multiple timescales.

In what follows, we provide a point-by-point response to the Reviewers' comments in **blue text**, alongside relevant excerpts from the revised manuscript (*italicised*, new material denoted in **boldface**). New content is also highlighted in **blue** in the accompanying revision.

Reviewer #1

Thank you to the authors for the in-depth consideration of the reviewers' comments and suggestions. The authors addressed all my concerns, and the manuscript is now much improved. Nevertheless, upon reading the revised manuscript, I noticed some methodological aspects that require clarification.

Thank you for your positive appraisal of our previous revision and response letter. We are happy to provide clarification on these additional points. Since many of these comments pertain to the reporting of statistical results (and the precise specification of the models from which these results were derived), we have opted to provide a new Supplementary Information containing summary tables for each reported GAMM.

1 - The mixed models used in most analyses provided a comparison between mind-wandering and on-task and mind-blanking and on-task but did not directly contrast mind-wandering and mind-blanking. These analyses and statistical results would be important to state that these two attentional states differ as claimed in the manuscript. This includes the analyses of the behavioral variables, heart rate, etc...

For example, in the discussion “Notably, mind-blanking states were accompanied by more pronounced reductions in heart-rate and response speed (RS_{μ}) than mind-wandering states, thus suggesting that mind-blanking is more likely to occur in the context of hypoarousal.” and “Mind-wandering, by contrast, was characterised by subtler decreases in arousal that were associated with more stereotypic behaviour (increased response bias (c), unchanged response speed) and increased heart-rate variability (HRV; as quantified by $IBlc_v$).” – these statements are not supported by statistical comparisons directly contrasting mind-wandering and mind-blanking.

We have altered our description of the HRV effect in our Discussion to reflect the fact that this difference is only significant relative to the on-task state – not mind-blanking (we are grateful to the Reviewer for helping us to see how our previous formulation could have been misconstrued):

*P.29: Notably, mind-blanking states were accompanied by more pronounced reductions in heart-rate and response speed (RS_{μ}) than mind-wandering states, suggesting that mind-blanking is more likely to occur in the context of hypoarousal (**although RS_{μ} was only significantly decreased in the Melbourne dataset**). Mind-wandering, by contrast, was characterised by subtler decreases in arousal that were associated with more stereotypic behaviour (increased response bias; c). **Furthermore, increased heart-rate variability (HRV; as indexed by $IBlc_v$) predicted the transition from task-focused attention to mind-wandering, and was also positively associated with pupil size, response bias, response speed, and false alarm rate [...]***

While reflecting on the Reviewer's suggestion that we report statistical contrasts of the two disengaged states, we realised that this could be elegantly accomplished by switching from treatment-coding to Helmert-coding. This contrast coding scheme enables us to test the difference between on-task vs. off-task states, and between mind-wandering vs. mind-blanking states. The relevant sections of the Results have been updated as follows:

*Pp.17-18: Using signal detection theory^{154, 155}, we calculated measures of sensitivity (d') and response bias (c) in 10 s trial epochs preceding thought probes. Sensitivity was significantly decreased in epochs preceding off-task reports as compared to on-task reports (**ON-OFF_{MEL}**: $\beta = 0.10$, s.e. = 0.01, $t = 6.61$, $p < .001$; **ON-OFF_{PAR}**: $\beta = 0.13$, s.e. = 0.01, $t = 11.16$, $p < .001$). Sensitivity tended to be lower during epochs associated with mind-blanking, although the difference between mind-blanking and mind-wandering was only significant in the Paris dataset (**MW-MB_{MEL}**: $\beta = 0.03$, s.e. = 0.03, $t = 1.01$, $p = .311$; **MW-MB_{PAR}**: $\beta = 0.06$, s.e. = 0.03, $t = 2.32$, $p = .020$). Bias was significantly increased (i.e., increased tendency to produce false alarms) in epochs preceding off-task reports in Melbourne but not Paris (**ON-OFF_{MEL}**: $\beta = -0.02$, s.e. = 0.01, $t = 2.55$, $p = .011$; **ON-OFF_{PAR}**: $\beta = -0.01$, s.e. = 0.01, $t = 1.16$, $p < .001$). However, both datasets revealed significant increases in response bias during mind-wandering compared to mind-blanking (**MW-MB_{MEL}**: $\beta = 0.03$, s.e. = 0.02, $t = 2.24$, $p = .025$; **MW-MB_{PAR}**: $\beta = 0.03$, s.e. = 0.01, $t = 2.43$, $p = .015$).*

*Mean response speed (1/Response Time; RS_{μ}) on Go trials did not significantly differ between on-task and off-task epochs (**ON-OFF_{MEL}**: $\beta = 0.01$, s.e. = 0.01, $t = 1.87$, $p = .061$; **ON-OFF_{PAR}**: $\beta = 0.01$, s.e. = 0.01, $t = 1.96$, $p = .051$). Mind-blanking was associated with slower response speed compared to mind-wandering, although this effect was only significant in Melbourne (**MW-MB_{MEL}** = 0.03, s.e. = 0.01, $t = 2.64$, $p = .008$; **MW-MB_{PAR}** = 0.01, s.e. = 0.01, $t = 1.19$, $p = .236$). Response speed variability (RS_{σ}) increased significantly from on-task to off-task reports (**ON-OFF_{MEL}**: $\beta = -0.03$, s.e. = 0.01, $t = 3.23$, $p = .001$; **ON-OFF_{PAR}**: $\beta = -0.02$, s.e. = 0.01, $t = 3.58$, $p < .001$). There was no significant difference in the variability expressed between mind-wandering and mind-blanking states in either dataset (**MW-MB_{MEL}**: $\beta = -0.03$, s.e. = 0.02, $t = 1.46$, $p = .144$; **MW-MB_{PAR}**: $\beta = -0.02$, s.e. = 0.01, $t = 1.56$, $p = .119$). See Table 1 for summary statistics; see **Supplementary Information for model summaries**.*

Please note, it is not possible to perform planned or post-hoc contrasts comparing mind-wandering and mind-blanking for models in which attentional state is the dependent variable – these models would need to be respecified with one of the off-task states set as the reference level. Note, however, that this additional testing is not necessary for the purposes of inferring a statistical difference in the probability of mind-blanking (vs. mind-wandering) as a function of mean IBI – this effect is evident from the coefficients plotted in Figure 2A, which show that the 95% confidence intervals for the mind-wandering and mind-blanking states do not overlap.

Importantly, the authors should specify their contrast coding choices for the categorical variables as explained in Laurel Brehm, Phillip M. Alday, Contrast coding choices in a decade of mixed models, Journal of Memory and Language, 2022
<https://www.sciencedirect.com/science/article/pii/S0749596X22000213>

We appreciate this point and have now made the contrast coding of categorical factors explicit in the Statistical Analysis section:

P.15: The contrast-coding scheme deployed in each GAMM was as follows: Categorical factors were Helmert-coded, enabling us to statistically evaluate the difference between engaged vs. disengaged states (i.e., on-task vs. the mean of mind-wandering and mind-blanking), and to directly contrast the difference between mind-wandering and mind-blanking. Since all other categorical variables consisted of only two levels, the interpretation of these fixed effects is the same as in sum-coding. Numeric predictors were unit-scaled unless already z-transformed (except for IBI index in the error-related cardiac deceleration model, which was included as an ordered factor with difference coding). Model intercepts encode the grand mean. See Supplementary Information for a full description of each GAMM.

2 - Maybe I am not quite understanding - the authors used the generalised additive mixed-effects model framework thereby controlling for performance fluctuations over time including effects of time-on-task. So, when the authors study the effect of the independent variable time-on-task (probe number) is this variable added to the model with the factor smooths? Wouldn't both capture the same variance?

Apologies for the confusion – it appeared in the original Statistical Analysis section that all models were equipped with factor smooths; however, the two models examining time-on-task effects were the exception. Instead, these models incorporated random intercepts for subject-level responses (essentially, they reduce to generalised linear mixed-effects models). This detail has been clarified as follows:

P.15: Since initial models regressing attentional state report onto probe number revealed a significant effect of time-on-task (see Results), all subsequent GAMMs were equipped with by-participant factor smooths on probe number to control for this effect, and to reduce the temporal autocorrelation of residuals (see Baayen and colleagues¹⁴⁴).

3 - In the section “Cardiac measures afford complementary information about arousal, attentional, and behavioural states”, in the reporting of the results from the mixed models, it is not clear which were the dependent and which were the independent variables? Please clarify.

For each of these analyses, cardiac indices were included as independent variables (i.e., predictors), hence our use of the term “predicted” when introducing and discussing these analyses (cf. Reviewer point 8):

P.19: Having confirmed the expected differences in behavioural and pupillometric measures across attentional and vigilance state reports, we next sought to examine whether cardiac parameters derived from epochs preceding thought probes could be used to predict attentional and arousal states and their behavioural correlates.

Please also note that Figure 2 depicts each of these effects in terms of the expected change in a given dependent variable for a unit increase in the focal cardiac parameter (i.e., the beta coefficients for mean IBI and IBI variability). We now explicitly include the term 'dependent variables' in the figure caption, and trust that any lingering uncertainty is assuaged by the Supplementary Information:

*Figure 2: Summary of modelled relationships between attentional, arousal, and behavioural state **dependent variables** and cardiac parameters. [...] See **Supplementary Information for full model summaries.***

4 - In the discussion: "Independent of attentional state, greater HRV predicted increased pupil dilation, response bias, response speed, and false alarm rate, suggesting transient fluctuations in heartbeat dynamics afford unique information pertaining to what appear to be impulsive patterns of behaviour." Independent of attentional state? Are these analyses run in independent models? Was attentional state and interactions included in the models? Which statistical analysis supports this statement?

We acknowledge that this wording is potentially ambiguous; we simply meant to indicate that some of our models revealed evidence of association between HRV, pupil size, and behavioural measures when attentional state was not taken into account. Essentially, all the models summarised in section "Cardiac measures afford complementary information about arousal, attentional, and behavioural states" comprised the same set of independent variables (site, stimulus type, mean IBI, IBI coefficient of variation, factor smooth on probe number by subject) – only the dependent variable was altered in each case. We have changed the wording of this passage to avoid the impression that we attempted to control for attentional state in these models (again, the potential for confusion is also mitigated by the transparent reporting of models in the Supplementary Information):

P.29: Furthermore, increased heart-rate variability (HRV; as quantified by IBI_{CV}) predicted the transition from task focus to mind-wandering, and was also positively associated with pupil size, response bias, response speed, and false alarm rate – suggesting that transient fluctuations in heartbeat dynamics afford unique information pertaining to what appear to be impulsive patterns of behaviour.

5 - In the section, "Error-related cardiac deceleration does not account for differences across attentional states", the sentence: "Interestingly, IBIs tended to be longer in sequences spanning a correct rejection ($F(1) = 23.95$, $p < .001$), irrespective of the subsequent attentional state report.", suggests the lack of an interaction between the effect of IBI order and attentional state report but this statistical result is not presented. Please complete the statistical reporting including all the effects and interactions included in the model.

Thanks for highlighting this; we have added the relevant (non-significant) statistical results:

P.22: Interestingly, IBIs tended to be longer in sequences spanning a correct rejection ($F(1) = 55.98$, $p < .001$), irrespective of the subsequent attentional state report ($F(2) = 1.69$, $p = .185$). This observation is consistent with our finding that increased IBI_{CV} predicted decreased

response bias. Moreover, false alarms did not generate more profound cardiac decelerations than those observed when responses to NoGo trials were **correctly withheld** ($|BI_{+1}| - |BI_0|$: **FA-CR = -0.02, s.e. = 0.02, $t = 1.04$, $p = .297$**).

Again, readers are directed to the Supplementary Information for a full description of the model:

*Model predictions are visualised in Figure 3 (see **Supplementary Information for summary tables**).*

6 - Analyses of HEP and MI were not controlled for time on task? This would be important as mind-wandering and blanking increase with time-on-task.

This is a good point; we now report results from modelling that included probe number as a second random effect to address the potential impact of time-on-task:

*P.16: Linear mixed-effects models including fixed effects of attentional state or vigilance level, covariates for stimulus type and site, and random intercepts for participant identity **and probe number**, were fitted to HEP or MI estimates using maximum likelihood estimation ('fitlme').*

Although this updated approach resulted in a reduction of the spatiotemporal extent of the significant HEP clusters reported in the previous version of the manuscript, the basic finding is still present:

*P.24: The first cluster spanned [388, **476**] ms, and included frontal and left-parietal scalp regions; the second cluster spanned [544, **564**] ms, and **was circumscribed to right fronto-temporal scalp regions** (Figure 4A).*

The findings of the mutual information analysis remain broadly the same as those reported in the previous submission. Figures 4 and 5 have both been updated to depict the results of the revised analyses.

7 - On the discussion of the HEP results: "Late modulation of heartbeat-evoked responses further indicates that mind-wandering is associated with amplified cortical processing of interoceptive signals." – the differences in amplitude of the HEP can only be interpreted as changes in the neural response. If we interpret the amplitude as a difference from zero it even suggests a decrease in absolute amplitude in the mind-wandering state and not otherwise, but given the overlap between different neural components possibly composing the HEP, this interpretation needs to be made with caution.

We agree that interpreting the functional significance of differences in ERP waveforms is often difficult and appreciate this note of caution. One of the difficulties here is the lack of a state-independent reference level – it is not clear whether the mind-wandering component reflects a reduced (i.e., closer to zero) vs. an enhanced (i.e., more negative-going) response, relative to the on-task component. We now replace the term "amplified" with "altered" to

maintain neutrality on the precise nature of the modulation, and emphasise its comparative nature:

*P.33: Late modulation of heartbeat-evoked responses further indicates that mind-wandering is associated with **altered** cortical processing of interoceptive signals **as compared to the on-task state**.*

8 - “Lower heart-rate predicted decreased vigilance and pupil size, in addition to slower, less-biased responses; increased heart-rate variability predicted more impulsive behaviour and pupil dilation.”

I suggest avoiding describing the results using the verb predict as for some variables the association is not obviously directional: for example, pupil size and cardiac measures are associated but either might predict the other.

We think our use of the term “predicted” is in keeping with general practices of reporting regression model effects – where the intention is to estimate the expected change in a response variable given a unit change in a predictor variable, while holding other predictors and covariates constant. While we agree it might not be wise to infer causal relations from such models, it is important in our view to recognise that the effects revealed by our models are ‘directional’ in the sense that the error term is located on the right side of the equation, and the interpretation of the predictor / beta coefficient of interest is conditional on the other variables included in the equation. In other words, our models are designed to address the question ‘can state y be predicted from cardiac parameter x’, not ‘can cardiac parameter x be predicted from state y’ – variables on either side of the equation cannot be treated equivalently (they are not exchangeable, as in correlation analysis).

9 – EEG preprocessing: Although possibly OK, running ICA twice after removing components is not advisable. Just a warning to avoid in the future. See: https://eeglab.org/tutorials/06_RejectArtifacts/RunICA.html

We agree with the view that it is not generally advisable to repeat ICA decomposition and component rejection procedures with the intention of achieving an even ‘cleaner’ dataset. Note, however, that our second ICA was not simply a repeat of the first – rather, it was specifically designed to target ECG-related components, insofar as it was deployed on R-peak time-locked epochs (rather than continuous records), and component rejection thresholds were determined by ECG-IC coherence (rather than the ICLabel routine). We are confident that the components rejected via this second procedure were dominated by ECG-related artefacts. We are unaware of any empirical work or conceptual argument against this specific approach, which has been deployed by a number of other teams working on ECG-EEG analysis (we provide citations to two such groups in the main text). We further note that there remains no consensus as to the best way of handling ECG-related artefacts in such analyses, although it is generally agreed that some attempt needs to be made to mitigate these sources of contamination and increase confidence that observed effects reflect genuine changes in the EEG.

Reviewer #3

The authors have responded to all my comments and concerns and made revisions accordingly. I am happy to recommend the revised manuscript for publication.

[Thank you for this endorsement.](#)