

Boredom signals deviation from a cognitive homeostatic set point.

Corresponding Author: Dr James Danckert

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Danckert,

Thank you for your patience during the peer-review process. Your manuscript titled "Cognitive Homeostasis: Boredom as a drive for optimal engagement" has now been seen by 2 reviewers, and I include their comments at the end of this message.

The reviewers are in principle enthusiastic about your work. However, they also mention a number of concerns. We are very interested in the possibility of publishing your manuscript in Communications Psychology, but would like to consider your response to these concerns in the form of a revised manuscript before we make a decision on publication.

Editorially, we consider it important to clarify how your account differs from existing proposals; here, the focus should be on the most notable characteristics of your proposal, rather than a comprehensive review of other models -- you may recommend the reader further familiarizes themselves with these by highlighting the pertinent references. Furthermore, rather than adding new simulations to your manuscript, we ask that you rely on existing literature to strengthen your account.

In sum, we invite you to revise your manuscript taking into account all reviewer and editor comments.

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Please use the following link to submit your revised manuscript and a point-by-point response to the referees' comments (which should be in a separate document to any cover letter):

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We hope to receive your revised paper within 12 weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Jennifer

Jennifer Bellintier, PhD
Senior Editor
Communications Psychology

REVIEWERS' EXPERTISE:

Reviewer #1 Boredom
Reviewer #2 Boredom

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors propose a feedback maintenance model of boredom, in which feelings of boredom arise from cognitive disengagement and drive re-engagement with one's activities and the world. While I agree with the authors' perspective, it wasn't clear to me how the proposed model deviates from those currently proposed in the literature. Indeed, most of the functional models described and many of the other models by the present manuscript's authors (meaning maintenance, boredom-as-feedback, etc) either explicitly or implicitly assume a homeostasis/feedback model of boredom. Because of this, it's somewhat unclear to me how the manuscript in its current form adds to the existing literature.

It would be helpful, in this regards, to clearly outline 1) what this new model claims, 2) what existing models claim, and 3) critically, what claims this new model makes that are NOT made by existing models in the literature. That is, how is this model distinct, and what does it bring to the table that others do not?

Along these lines, while a theory paper doesn't need to provide a lot of data, providing some empirical evidence for why this approach should be favored over other alternative theoretical approaches would go a long way.

Finally, a good model not only makes sense of the "facts on the ground," but also goes beyond those facts to make testable hypotheses - that is, a good model is both comprehensive and generative. It would be helpful to outline what specific hypotheses this model generates that diverge from other existing models, and how such hypotheses could be tested. Such an approach would not only help distinguish (empirically) this theoretical model from others, but also provide a useful source of theoretically-driven novel research questions that could drive future research in the field. The paper does this to some extent in pages 11-13; there is very little existing neuroscience work looking at boredom, and the authors propose some deeply interesting ideas (driven implicitly by the model) for future research in this area.

Reviewer #2 (Remarks to the Author):

Review for „Cognitive Homeostasis: Boredom as a Drive for Optimal Engagement“

Thank you for the opportunity to review this insightful and highly relevant manuscript. The authors present a compelling theory, positioning boredom as a self-regulatory signal to restore cognitive engagement and achieve a cognitive "set-point." This approach contributes to the ongoing exploration of boredom as an adaptive mechanism rather than a passive state. The manuscript is well-organized and addresses timely questions with testable hypotheses regarding boredom and cognitive engagement. Below, I outline a few key areas that could strengthen the manuscript by expanding on related literature, refining theoretical distinctions, and clarifying mechanisms to further improve the argument presented. I hope these are seen as helpful in further improving the paper.

Major Points

Integration of boredom conceptualizations (and boredoms interplay with effort and self-control)

I really like that the manuscript effectively positions and defines boredom as a self-regulatory signal because I feel this aligns very well with recent work from our group on the definition of boredom

(<https://www.taylorfrancis.com/chapters/edit/10.4324/9781003271536-3/different-wanja-wolff-vanessa-radtker-corinna-martarelli>), as well as on (state and trait) boredoms relationship with effortful control (<https://pubmed.ncbi.nlm.nih.gov/33899724/>), and self-control (<https://pubmed.ncbi.nlm.nih.gov/32697921/>, <https://www.taylorfrancis.com/chapters/edit/10.4324/9781003271536-11/virtues-fragile-self-control-maik-bieleke-wanja-wolff-alex-bertrams>). It is encouraging to see thoughts on the topic converge across independent axis of thought. To illustrate, in (<https://www.taylorfrancis.com/chapters/edit/10.4324/9781003271536-3/different-wanja-wolff-vanessa-radtker-corinna-martarelli>), we define boredom as an indication of "inadequate function utilization" that occurs when a zone where task demands meet preferences/capacities/energy levels is left. My feel is that this complements the homeostasis framework presented here very well by offering an inverse perspective. Specifically, we suggest that boredom drives exploration by highlighting situations of cognitive underutilization (low entropy conditions), which aligns with but also broadens the understanding of boredom's regulatory role. As a further complement to the account present here, Wolff et al also provides an idea why boredom might not have an easily identifiable signature and a strong link to a specific arousal configuration. Lastly, integrating the predictive coding perspective in Wolff et al., (which ties boredom to minimized reward prediction error) could clarify how boredom might both stabilize cognitive engagement and promote adaptive exploration when needed in the present paper. Please note that I am by no means asking you to cite our work in the present paper; especially if it does not substantially advance the arguments presented here. I am just personally curious about the authors' thoughts on these parallels.

Definitions and Contemporary Theories of Effort (and its allocation and valuations)

I found it very convincing to also focus on effort to some degree in the paper (especially given our own groups interest in the topic in this context :)). However, I think the paper could improve its handling of "effort" by drawing on frameworks that address effort allocation more explicitly in terms of an optimization process. For example the Expected Value of Control Theory (<https://pmc.ncbi.nlm.nih.gov/articles/PMC3767969/>) or other such frameworks that emphasize that effort is not minimized per se, but rather that one tries to achieve the best cost/benefit ratio. In my reading, this is a shared assumption by various contemporary such theories (<https://pubmed.ncbi.nlm.nih.gov/35679204/>). Such theories underscore how effort costs and rewards are dynamically evaluated, directly informing one's decision to engage (thereby also prefacing the function of effort that is proposed in Wolff & Martarelli, 2020 or in Agrawal et al., 2022). This literature could deepen the paper's theoretical grounding, offering a more nuanced view of effort's role in maintaining cognitive homeostasis.

The term "resource utilization" might also benefit from a little more definition and discussion, as it implies specific cognitive capacities that are utilized or underutilized without clarifying what constrains performance. An explicit definition here would clarify how under- or over-utilization contributes to boredom within this model. In terms of terminology in this context (effort, resources, utilization), I feel Halperin & Vigotsky's recent definition piece on effort might be a useful:

<https://pubmed.ncbi.nlm.nih.gov/38909350/>

Regarding effort-demanding tasks that don't feel effortful. I think it is important to differentiate between feeling effortless and valancing the experience of effort differently here. Recent years have seen a surge in research on the idea that effort can be enjoyable/valuable in its own right (see for example The Effort Paradox: Effort Is Both Costly and Valued - PMC). For example, there is research from us and users displaying individual differences in how effort is valued () and we found that people who value (specific for cognitive and physical) effort find effortful activities less boring (<https://pubmed.ncbi.nlm.nih.gov/38909350/>), which is fully consistent with the arguments you have laid out here.

Neuroscience in cognitive setpoint research

One thing that might be considered here too is the fleetingness of psychological states, such as boredom, no? To illustrate, boredom, can be abruptly realized and then produce something. For example rumination about the boring situation, mind-wandering. It might also prompt frustration and the urge to escape. Or one might "give up" and become apathetic in response to it. So, low resolution measures might often conflate the boredom and the response to it, no? As the authors emphasize, this would call for super careful event-related high resolution matching of self-reports and neuro-data. Its probably the same with EEG frequencies analyses too, right? if an episode is boring and we see frequency differences compared to control condition, then this probably does not imply that the frequency difference represents purely "boredom" but rather "boredom and the various responses and fluctuations it might beget". i.e., the brain is most likely not truly engaged with the task but also not exclusively bored but busy with being frustrated at the task? This might then be a departure from a non-boring setpoint, which potentially can take any shape or form, depending on the pattern of disengagement, no? Apologies for this windingly long comment, my point is: maybe expand (even) more on the idea that what we see in neuroscience data might be boredom but also the response to it. and the response can be varied.

Minor Points

The authors write "boredom's ultimate goal": while I like the playful language here, this might anthropomorphize psychological states in a way that detracts from the paper's clarity. Reframing as "boredom signals" or "indicates" could avoid suggesting intentionality in such states.

Cognitive Setpoint: I wonder if boredom is only occurring when deviation from a cognitive setpoint is detected? Could there be also an emotional or a motivational setpoint and could deviations from this also prompt boredom (and other states)?

The authors write "Specifically, utilization of cognitive resources can be said to depend on the capacity to extract a signal from noise": I probably don't understand this completely but wouldn't utilization imply that resources are expended, irrespective of the capacity of being "good at it"? [for a similar use of this term, please see also The Halperin & Vigotsky paper further up]

The authors write "Departures from this set-point lead to negative consequences both in terms of task performance (e.g., poor attentional control)": I wonder if poor attentional control truly is a negative consequence of set point departure in this context or rather the boredom-induced attempt to find better things to engage with (i.e., boredom directing attention elsewhere in the hope to get arrive at the setpoint again)? Effortful self-control is then likely used to keep one's thought engaged with the task at hand and failures/unwillingness to deploy enough control (in terms of EVC) then lead to attentional slips actually happening (boredom wins). Curious to hear the authors take on this idea (again, no need to add to the paper, if they deem this too far off the original idea they are trying to present).

Flow and its many characteristics: Not super important here but it might be worthwhile to also refer to substantial critiques on the flow concept (in terms it being undertheorized and with respect to relevance of these flow dimensions) here? See for example chapter 8 in Rethinking Sport and Exercise Psychology Research: Past, Present and Future | SpringerLink. To conclude, I really like the present study and I hope my comments can help the authors to improve the paper even further. I am looking forward to reading a revision of this paper!

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Version 1:

Decision Letter:

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Dear Dr Danckert,

Your manuscript titled "Cognitive Homeostasis: Boredom as a drive for optimal engagement" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Psychology.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers and a list of editorial requests. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Jennifer Bellingtier

Jennifer Bellingtier, PhD
Senior Editor
Communications Psychology

REVIEWERS' EXPERTISE:

Reviewer #2 Boredom

REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

I really like the job the authors have done in revising the paper (and in answering to the reviewer comments). I believe this has made the ideas (even) more precise and the novelty of the model more apparent. Congratulations to this very interesting piece!

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Response to Reviews**Reviewer 1's comments:**

R1.1: Clearly outline what this new model claims

Authors' response

We thank the reviewer for this suggestion. A crucial feature of our model—and one that we believe distinguishes it from extant models—is the involvement of allostatic processes. These processes play a triple role in our model.

First, allostatic processes confer flexibility to the parameters of cognitive involvement and specifically, to what counts as an acceptable (or desired) range of cognitive engagement.

Second, the inclusion of allostatic processes gives our model of boredom an anticipatory component so that it is not merely reactive.

Third, the involvement of *both* allostatic and homeostatic processes allows us to make progress in understanding the complex relationship between state and trait boredom (or boredom proneness).

To clarify our claim outlined above, we have extended the introduction on pages 3-4.

In addition, we see this model as having broader implications beyond boredom that relate to the mechanisms needed to regulate cognitive-affective function writ large. We make this point when we outline how our model is distinct (pp. 3-4) and in the conclusion (p. 17).

R1.2: Clearly outline what existing models claim

As mentioned in response to comments R1.1 and R1.3 we expanded our outline of what our current model adds to the field. Specifically, we added a short paragraph outlining briefly how our model is differentiated from other models. These other models include the Boredom Feedback Model (Tam et al., 2021)—which presents inadequate attentional engagement as a trigger to attentional shifts—and the Meaning and Attentional Components model (Westgate & Wilson, 2018)—which suggests that boredom arises as an indicator of a mismatch between current activities and valued goals for the meaning component, and between cognitive resources and demands for the attentional component (p. 4).

R1.3: Critically, what claims this new model makes that are NOT made by existing models in the literature. That is, how is

We thank the reviewer for pointing out the need for further clarification. We have extended the introduction paragraph on pages 3-4 to directly highlight what differentiates our proposed model (see response to R 1.1).

this model distinct, and what does it bring to the table that others do not?

R1.4: Provide some empirical evidence for why this approach should be favored over other alternative theoretical approaches

We thank the reviewer for this suggestion. We added further evidence that supports our approach at the top of page 5, which highlights work from Westgate & Wilson (2018) and Struck et al (2020) showing boredom to be highest at both low and high ends of either task difficulty or perceived control respectively. We see this work as initial indications that boredom acts to keep us within an optimal range of cognitive engagement.

Our current model also spans functional descriptions of the state of boredom *and* characterizations of the trait disposition to be boredom prone in ways that prior models do not. That is, the combination of homeostatic and allostatic accounts of boredom (see response to R1.1 above) more comprehensively describes the phenomenon and allows for direct tests (particularly within a predictive coding account). We have emphasized this difference in the revised manuscript (pp. 3-4), discussed the admittedly sparse empirical evidence on p. 5, and provide more potential hypotheses on p.13.

R1.5: A good model is both comprehensive and generative. It would be helpful to: outline what specific hypotheses this model generates that diverge from other existing models, and; how such hypotheses could be tested.

We thank the reviewer for these excellent suggestions. We have supplemented the hypotheses and methods provided in the original manuscript (pp. 12-13) with several brief hypotheses (p. 13). We did so in the context of recent work highlighting the role of the insular in affective regulation (Feldman et al., 2024).

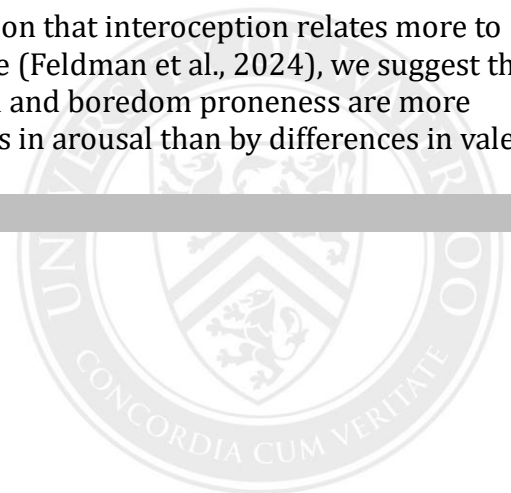
That is, the added hypotheses are based on recent work showing that people who are more boredom prone tend to be hyperaware of interoceptive signals, while simultaneously struggling to make sense of those signals (Trudel et al., 2024). Given the claim that interoception may rely more on model predictions than on prediction error (Feldman et al., 2024), we suggest that it may be the case that boredom prone individuals have poor model-based predictions.

In addition, given the suggestion that interoception relates more to ratings of arousal than valence (Feldman et al., 2024), we suggest that it may be the case that boredom and boredom proneness are more strongly driven by fluctuations in arousal than by differences in valence.

Reviewer 2's comments:

Major Points

R2.1. Integration of



boredom conceptualizations (and boredom interplay with effort and self-control) - As a further complement to the account present here, Wolff et al also provides an idea why boredom might not have an easily identifiable signature and a strong link to a specific arousal configuration. Lastly, integrating the predictive coding perspective in Wolff et al., (which ties boredom to minimized reward prediction error) could clarify how boredom might both stabilize cognitive engagement and promote adaptive exploration when needed in the present paper.

R2.2: Definitions and Contemporary Theories of Effort (and its allocation and valuations)

R2.2a: The paper could improve its handling of “effort” by drawing on frameworks that address effort allocation more explicitly in terms of an optimization process. For example the Expected Value of Control Theory (<https://pmc.ncbi.nlm.nih.gov/articles/PMC3767969/>) or other such frameworks that emphasize that effort is

We thank the reviewer for pointing out this research which is clearly relevant to our model. We agree that there are many parallels and have given brief mention to the role of self-control (or as the reviewer calls it effortful control) on p. 10. We agree that in circumstances of underutilization that boredom may drive exploratory behaviours (while at higher ends of information overload it may prompt retreat to more predictable environs). We feel that Section 2 highlights the main thrust of our model, which is that boredom does indeed signal deviation from optimal engagement, while self-control—or self-regulation writ large—is likely one of the underlying mechanisms that influences how effectively we stay within the optimal zone. We felt that we did not have the space to do this argument justice. As such, we point to the Wolff et al (2022) chapter for readers interested in this aspect of boredom and self-control.

We thank the reviewer for providing feedback and relevant references to aid in better defining the terms used in our model.

We have expanded our account of “Effort” on p. 6.

While we did touch briefly on the Control-Value Theory in the original manuscript and implicitly addressed effort in the context of challenging (effortful) and underchallenging (easy) tasks in educational settings (page 5), we have now added discussion of the Expected Value of Control Theory on page 9.



not minimized per se, but rather that one tries to achieve the best cost/benefit ratio. In my reading, this is a shared assumption by various contemporary such theories

(<https://pubmed.ncbi.nlm.nih.gov/35679204/>).

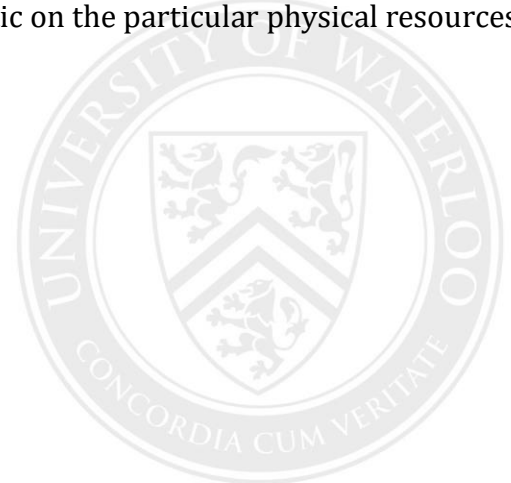
Such theories underscore how effort costs and rewards are dynamically evaluated, directly informing one's decision to engage (thereby also prefacing the function of effort that is proposed in Wolff & Martarelli, 2020 or in Agrawal et al., 2022).

This literature could deepen the paper's theoretical grounding, offering a more nuanced view of effort's role in maintaining cognitive homeostasis.

R2.2b: The term “resource utilization” might also benefit from a little more definition and discussion, as it implies specific cognitive capacities that are utilized or underutilized without clarifying what constrains performance.

An explicit definition here would clarify how under- or over-utilization contributes to boredom within this model. In terms of terminology in this context (effort, resources, utilization), I feel Halperin & Vigotsky's recent definition

To address R2.2b, we clarified the definition of “resource” on page 9. In particular, after reviewing Halperin & Vigotsky (2024) we wanted to ensure that we were clear that we are using the term cognitive resource here broadly to refer to something that can be utilized to complete cognitive activities, rather than some internal limited supply that is consumed by (possibly depleted by) particular classes of cognitive acts (see Inzlicht & Schmeichel, 2012; Halperin & Vigotsky, 2024). Thus, utilization would reflect the use of the former class of cognitive resource (i.e., completing cognitive actions). Our proposal, at present, is agnostic on the particular physical resources that undergird cognition.



piece on effort might be a useful: <https://pubmed.ncbi.nlm.nih.gov/38909350/>

R2.2c: Regarding effort-demanding tasks that don't feel effortful. I think it is important to differentiate between feeling effortless and valancing the experience of effort differently here.

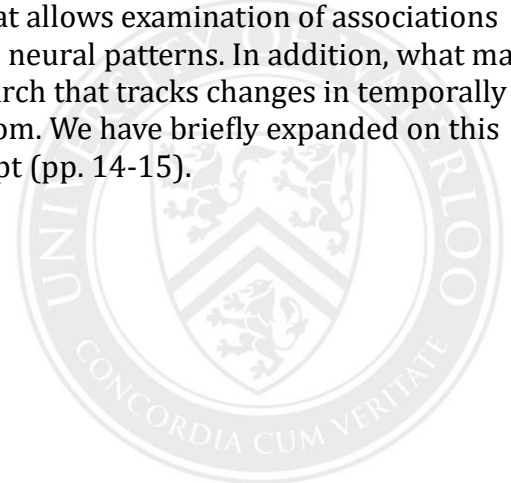
Recent years have seen a surge in research on the idea that effort can be enjoyable/valuable in its own right (see for example The Effort Paradox: Effort Is Both Costly and Valued - PMC). For example, there is research from us and users displaying individual differences in how effort is valued () and we found that people who value (specific for cognitive and physical) effort find effortful activities less boring (<https://pubmed.ncbi.nlm.nih.gov/38909350/>), which is fully consistent with the arguments you have laid out here.

R2.3: Neuroscience in cognitive setpoint research: expand (even) more on the idea that what we see in neuroscience data might be boredom but also the response to it. and the response can be varied.

We thank the reviewer for bringing forth such an important distinction. We had initially attempted to address this when discussing the notion of cognitive miserliness (pp. 5-6). We enjoyed reading the reviewer's work on individual differences in valuing physical and mental effort and have made brief mention of it in parentheses in the revised manuscript (p. 6).

We have expanded this in the revised manuscript (pp. 6, 9 and 10).

We thank the reviewer for his suggestion. We agree that, thus far, neuroimaging data from studies that directly examined boredom may reflect the state itself, people's in-the-moment responses to the state, or some combination of both. As suggested, what is needed is high (temporal) resolution data that allows examination of associations between subjective states and neural patterns. In addition, what may prove fruitful is imaging research that tracks changes in temporally dynamic states such as boredom. We have briefly expanded on this point in the revised manuscript (pp. 14-15).



Minor Points

R2.4: The authors write “boredom’s ultimate goal”: while I like the playful language here, this might anthropomorphize psychological states in a way that detracts from the paper’s clarity. Reframing as “boredom signals” or “indicates” could avoid suggesting intentionality in such states.

We thank the reviewer for this point and have deleted the sentence in question (p.2).

R2.5: Cognitive Setpoint: I wonder if boredom is only occurring when deviation from a cognitive setpoint is detected? Could there be also an emotional or a motivational setpoint and could deviations from this also prompt boredom (and other states)?

We agree that there are likely other ‘set-points’ for optimal engagement including motivation and affect. With respect to motivation, abundant work shows that intrinsic, as opposed to extrinsic, motivation works best to achieve engagement. In terms of affect, Feldman Barrett and colleagues suggest that affective predictive coding plays a critical role in self-regulatory processes. We do give some space to discussion of this account (p. 12). In the interest of adhering to the space constraints we have simply added intrinsic motivation as a potential signal of deviation from an optimal setpoint (along with meaning, boredom and effort) on p 10.

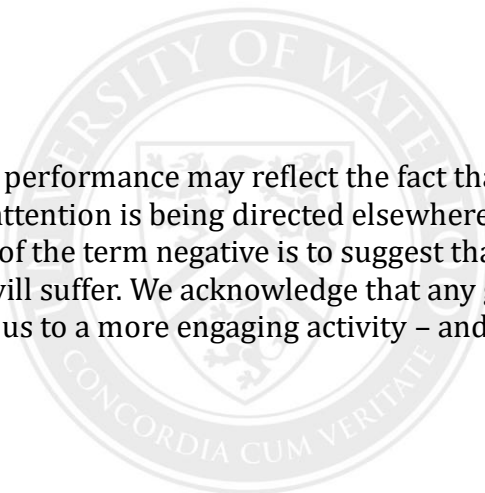
R2.6: The authors write “Specifically, utilization of cognitive resources can be said to depend on the capacity to extract a signal from noise”: I probably don’t understand this completely but wouldn’t utilization imply that resources are expended, irrespective of the capacity of being “good at it”? [for a similar use of this term, please see also The Halperin & Vigotsky paper further up]

We agree that utilization implies that resources are expended. What we meant to say is that that the extent to which resources are *optimally deployed or not* will depend on the ability to extract signal from noise. In that sense, this is less about effort and more about effective information processing (à la Klapp, 1986). While Halperin and Vigotsky discuss effective and ineffective effort, here we are concerned with making sense of information.

We have attempted to clarify this in the revised manuscript (p. 4).

R2.7: The authors write “Departures from this set-point lead to negative consequences both in terms of task performance (e.g.,

We agree that poor attentional performance may reflect the fact that a bored state has arisen or that attention is being directed elsewhere in response to boredom. Our use of the term negative is to suggest that in those instances performance will suffer. We acknowledge that any given episode of boredom may drive us to a more engaging activity – and as



poor attentional control)”: I wonder if poor attentional control truly is a negative consequence of set point departure in this context or rather the boredom-induced attempt to find better things to engage with (i.e., boredom directing attention elsewhere in the hope to arrive at the setpoint again)? Effortful self-control is then likely used to keep one's thought engaged with the task at hand and failures/unwillingness to deploy enough control (in terms of EVC) then lead to attentional slips actually happening (boredom wins). Curious to hear the authors take on this idea (again, no need to add to the paper, if they deem this too far off the original idea they are trying to present).

R2.8: Flow and its many characteristics:

it might be worthwhile to also refer to substantial critiques on the flow concept (in terms it being undertheorized and with respect to relevance of these flow dimensions) here? See for example chapter 8 in Rethinking Sport and Exercise Psychology Research: Past, Present and Future | SpringerLink.

such, the signal and its consequences for attention on the task at hand (which for performance are negative), need not be considered negative in totality. We have added a brief discussion of this on p. 7.

Given space constraints we have chosen to make a brief mention of these critiques and have directed the reader to the further resources suggested by R2 (p. 11).

