JOURNAL OF Neuroscience Research

**Post-training Stimulation of the Right Dorsolateral Prefrontal Cortex Impairs Working Memory Training Performance**

Jacky Au1, Benjamin Katz, Austin Moon, Sheebani Talati, Tessa R. Abagis, John Jonides,Susanne M. Jaeggi

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Revision Received: 17 December 2020
Accepted: 23 December 2020

Editor 1: Junie Warrington
Editor 2: Elizabeth Johnson
Reviewer 1: Elizabeth Johnson
Reviewer 2: Donel Martin

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1st Editorial Decision

Dear Dr Jaeggi:

Thank you for submitting your manuscript to the Journal of Neuroscience Research. We have now received the reviewer feedback and have appended those reviews below. As you will see, the reviewers find the question addressed to be of potential interest. Yet, they do not find the manuscript suitable for publication in its current form.

If you feel that you can adequately address the concerns of the reviewers, you may revise and resubmit your paper within 90 days. It will require further review. Please explain in your cover letter how you have changed the present version. If you require longer than 90 days to make the revisions, please contact Dr Junie Warrington (jpwarrington@umc.edu). You can submit your revised manuscript directly by clicking on the following link: *** PLEASE NOTE: This is a two-step process. After clicking on the link, you will be directed to a webpage to confirm. ***

https://mc.manuscriptcentral.com/jnr?URL_MASK=cafec9e080ea434f88f9961ce7e1b58d

Thank you again for your submission to the Journal of Neuroscience Research; we look forward to reading your revised manuscript.

Best Wishes,

Dr Elizabeth Johnson
Associate Editor, Journal of Neuroscience Research

Dr Junie Warrington
Editor-in-Chief, Journal of Neuroscience Research

Editorial Comments to Author:

JOURNAL OF Neuroscience Research

1. Please upload a graphical abstract, which we are asking of all authors submitting original research articles. This is intended to provide readers with a visual representation of the conclusions and an additional way to access the contents and appreciate the main message of the work. What we require is a .tif image file and a .doc text file containing an abbreviated abstract. For the image, labels, although useful, must be kept to a minimum and the image should be 400 x 300, 300 x 400, or 400 x 400 pixels square and at a resolution of 72 dpi. This can be one of the figures from your article, or something slightly different, as long as it represents your study. Instructions for this can be found in our author guidelines online at http://onlinelibrary.wiley.com/journal/10.1002/(ISSN)1097-4547/homepage/ForAuthors.html

2. Please add to your paper (after the Discussion and Acknowledgments, immediately before the References) a conflict of interest statement and a statement of authors' contributions. The statement must follow the CRediT Taxonomy. You can find examples of such statements in the author guidelines on-line at http://onlinelibrary.wiley.com/journal/10.1002/(ISSN)1097-4547/homepage/ForAuthors.html

3. Authors must submit, in the main text of the document, a 100-word-maximum statement about the significance of their research paper written at a level that is understandable to the general public and to scientists outside their field of specialty. This statement will be distinct in purpose from the abstract, with the primary goal of broadly explaining the relevance and importance of this work and how this work contributes to different diseases.

4. Please be sure all continuous data plots are depicted as scatterplots or box and whisker plots with dot plot overlay rather than bar graphs to better visualize the distribution of data.

5. To enable readers to locate archived data from Journal of Neuroscience Research papers, we require authors to include a 'Data Accessibility' section just before the References. This should list the database(s) and URL(s) or dataset DOIs for all data associated with the manuscript. Data deposit repositories might include unstructured repositories such as Dryad, FigShare, NeuroMorpho or centralized repositories from the institutions in which the research was conducted. We also strongly recommend depositing data in the Open Science Framework. JNR will also allow small data sets to be included as Supplementary Files with the article.

6. Please include your figure legends in a separate Figure Legends section at the end of your Microsoft Word text file. Currently, your legends are included as captions in the figures themselves, which is helpful during the review phase, but these captions do not appear in the final published version of the manuscript.

Associate Editor: Johnson, Elizabeth
Comments to the Author:
Dear Dr. Au,

Enclosed are the views of the experts to whom we turned for evaluation of your manuscript. The reviewers thought that this study would contribute to our understanding of how tDCS affects cognition, but also noted several serious problems which require your attention.

In general, both reviewers found the tDCS methods and literature review insufficient. Reviewer 1 also found issues in the interpretation of neural substrates and relevance for the special issue. Reviewer 2 found issues with statistical analysis. In addition to their comments, we suggest that data figures would benefit from inclusion of individual data points.

If you are willing to undertake the modifications and clarifications requested, please do so at your earliest convenience. Naturally, you are free to rebut these reviews should you believe them to be in error, but to do so will require significant and substantive information to allow us to reject the reviewers' statements.

Thank you for your submission,
Drs. Elizabeth Johnson and Kevin Jones

Reviewer: 1

Comments to the Author
jnr-2020-Jul-8890

This study reports a decrease in WM performance following paired WM training and DLPFC tDCS when stimulation was applied after training. Performance enhancement was observed with training but not as a function of treatment (online, offline before, or offline after vs. sham). The authors suggest that post-training tDCS promoted memory interference to the detriment of WM performance, and emphasize the importance of stimulation parameters in protocols aimed at cognitive enhancement. Whereas these results do show potentially intriguing changes in cognitive performance within individuals, minimal attention is paid to neural substrates. The authors should reframe this manuscript to meet the focus of the special issue.

Additional feedback:

The hypothesis was that previous enhancement effects following online tDCS would be replicated, but they were not. The reader is left wondering about this from the abstract until the discussion, when it is introduced that the stimulation site differed from the previous study (p. 19). This should be considered especially pertinent given the focus of the special issue.

Online tDCS was expected to "facilitate training performance in a baseline-dependent manner, with greater gains over the weekend compared to consecutive weekdays…" (p. 5). However, participants were matched on both baseline WM performance and starting day so that the weekend fell at a consistent point. These procedures seem to conflict with the hypothesis.

Why was right DLPFC selected at the stimulation site? Details on study rationale are needed to contextualize this study. This is especially confusing as the abstract indicates a negative role for DLPFC in memory, yet the introduction only indicates previously observed benefits of DLPFC stimulation. It shouldn't be assumed that readers are familiar with previous work by this group.

Same goes for the n-back task design (p. 8).

More details are needed about stimulation parameters. For instance, where were the anode and cathode placed? What was the "shortcut algorithm" used to target DLPFC (p. 9)? What was the sham stimulation procedure?

Figure S1 shows maximal electric field intensity at bilateral frontal pole, not right DLPFC. This is critical. It is also indicated in the text that this figure does not show the polarity of current flow – why not? Perhaps a more detailed model would reveal current flow from right DLPFC to frontal pole – or should this paper be about the frontal pole? And, given the focus of the special issue, the current modeling figure should not be supplemental.

Analyses showed: (1) a session effect reflecting performance enhancement regardless of tDCS treatment; (2) a session x treatment interaction such that the post-training group improved the least; and (3) the post-training group showed stronger gains after the weekend relative to a weekday. These results do not seem to support a detrimental effect of post-training tDCS, but rather relatively less benefit.

Further, what should readers make of the post-training weekend effect? Should spacing out training be considered a possible remediation of post-training DLPFC stimulation effects? How might this make sense considering the brain?

Given the focus of the special issue, the authors should consider discussing WM performance enhancement in the context of published training studies that included neuroimaging measures.

The discussion of left DLPFC stimulation as beneficial and right DLPFC stimulation as detrimental suggests the problem is the right hemisphere. However, there is literature showing that right-hemisphere tDCS improves WM.

Finally, the discussion of right DLPFC in memory interference does not necessarily explain the results. Although it is reasonable to consider shared neural mechanisms between WM and long-term memory, the n-back study-test trials are fully contained within a session. So, what would be interfered with after an n-back session? This differs from a long-term memory experiment with study and test separated by stimulation potentially promoting interference.

Reviewer: 2

JOURNAL OF Neuroscience Research

Comments to the Author
This controlled trial investigated the effect of timing of tDCS on repeated working memory training. The study attempted to replicate results from a prior study (Au et al., 2016) though was unable to show a significant effect of online relative to sham tDCS in facilitating training performance unlike the prior study. Interestingly, they found instead that offline tDCS administered after training impaired learning and consolidation of training effects. The results are interesting and represent an important addition to the field.

I have though the following concerns/queries regarding the paper in its current form.

MAJOR
1. Statistical analyses. Many post hoc analyses were conducted with unclear rationale. These further do not significantly contribute to the main results.
2. Statistical analyses. Table 1. It is not clear why the models were repeated using the offline post training condition as the reference. This does not add anything to the main results presented in the Table with the sham reference (which was consistent with the addressing the stated hypothesis).

MINOR
1. The Introduction would benefit from citing other relevant papers in this field (Jones et al., 2015, Plos One, Martin et al., 2013, Int J Neuropsychopharm, Richmond et al., 2014, J Cog Neurosci).
2. Methods. Was a power analysis conducted? If so, this should be described as effects from tDCS are typically small.
3. Please describe the stimulation montage and parameters in more detail, including sham.
4. Results. Please describe the demographics of the 3 groups, i.e., age, gender, education. These factors can potentially affect outcomes.
5. As the main effect of condition for the ANOVA analysing the 1 month effects was non-significant, this indicates that there was no difference between conditions. The results should be interpreted as such in the Discussion, i.e., page
16._____
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Authors' Response

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We thank the reviewers for their helpful feedback, which we believe has greatly strengthened the manuscript. We have made significant revisions which address the concerns of both the reviewers as well as the editor. Significant changes in the manuscript are highlighted in red. We hope you find the manuscript much improved. Below are our point-by-point responses in bold:

## Editorial Comments:

1. Please upload a graphical abstract, which we are asking of all authors submitting original research articles. This is intended to provide readers with a visual representation of the conclusions and an additional way to access the contents and appreciate the main message of the work. What we require is a .tif image file and a .doc text file containing an abbreviated abstract. For the image, labels, although useful, must be kept to a minimum and the image should be 400 x 300, 300 x 400, or 400 x 400 pixels square and at a resolution of 72 dpi. This can be one of the figures from your article, or something slightly different, as long as it represents your study. Instructions for this can be found in our author guidelines online at http://onlinelibrary.wiley.com/journal/10.1002/(ISSN)1097-

4547/homepage/ForAuthors.html
**This has been added.**

2. Please add to your paper (after the Discussion and Acknowledgments, immediately before the References) a conflict of interest statement and a statement of authors' contributions. The statement must follow the CRediT Taxonomy. You can find examples of such statements in the author guidelines on-line at http://onlinelibrary.wiley.com/journal/10.1002/(ISSN)1097-4547/homepage/ForAuthors.html

**This has been added.**

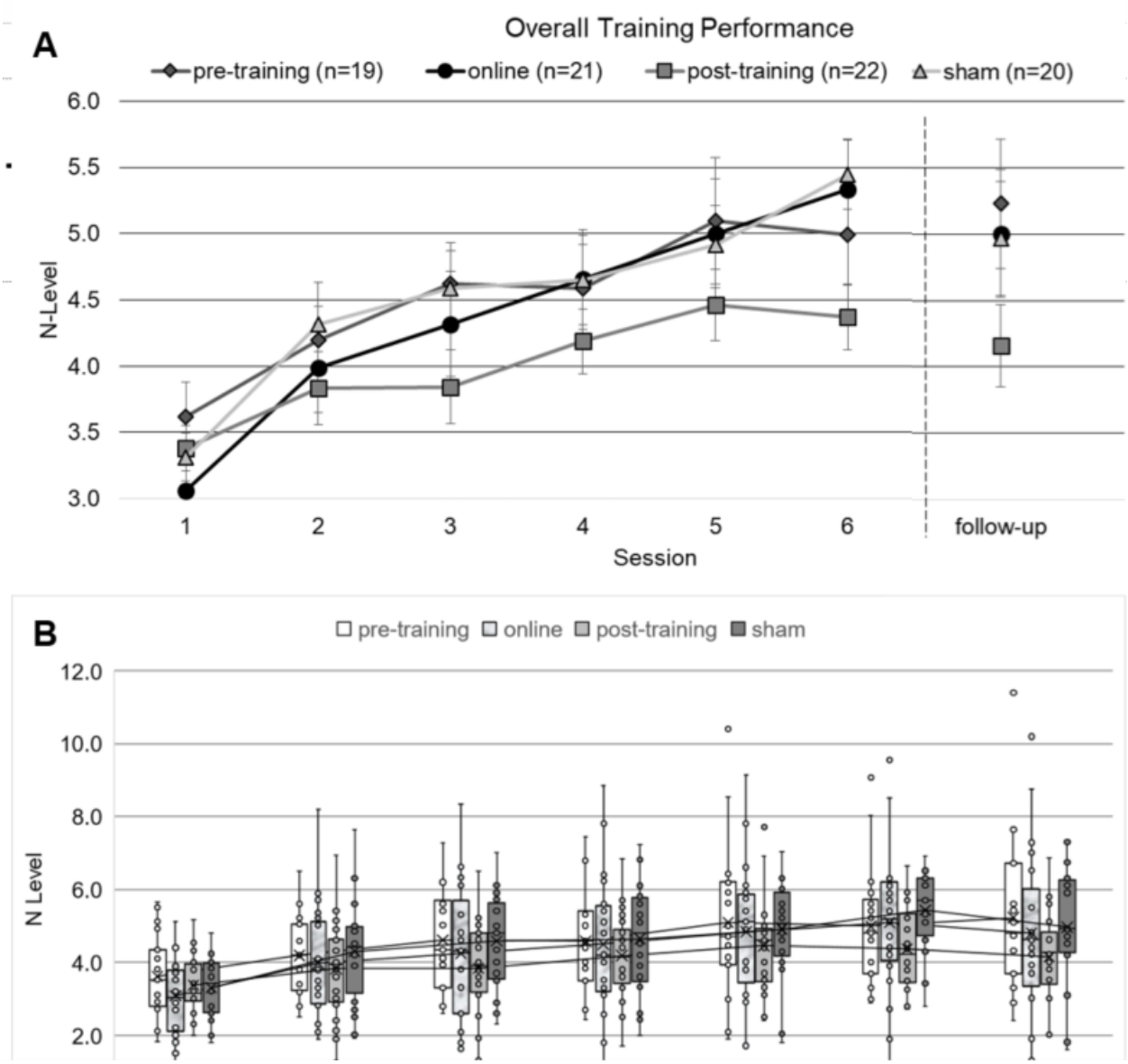
3. Authors must submit, in the main text of the document, a 100-word-maximum statement about the significance of their research paper written at a level that is understandable to the general public and to scientists outside their field of specialty. This statement will be distinct in purpose from the abstract, with the primary goal of broadly explaining the relevance and importance of this work and how this work contributes to different diseases.

**This has been added.**

4. Please be sure all continuous data plots are depicted as scatterplots or box and whisker plots with dot plot overlay rather than bar graphs to better visualize the distribution of data.

**This has been done for Figure 4, but we have decided to keep Figure 3 as a line graph since there is too much data to visualize properly as box plots. We have included both figures below for your review. We hope you agree that the line graph is more visually appealing, but we will be happy to adjust the figure at your**

**discretion.**




5. To enable readers to locate archived data
from Journal of Neuroscience Research papers, we require authors to include a 'Data Accessibility' section just before the References. This should list the database(s) and URL(s) or dataset DOIs for all data associated with the manuscript. Data deposit repositories might include unstructured repositories such as Dryad, FigShare, NeuroMorpho or centralized repositories from the institutions in which the research was conducted. We also strongly recommend depositing data in the Open Science Framework. JNR will also allow small data sets to be included as Supplementary Files with the article.

**This has been added.**

6. Please include your figure legends in a separate Figure Legends section at the end of your Microsoft Word text file. Currently, your legends are included as captions in the figures themselves, which is helpful during the review phase, but these captions do not appear in the final published version of the manuscript.

**This has been added.**

**Reviewer 1**

This study reports a decrease in WM performance following paired WM training and DLPFC tDCS when stimulation was applied after training. Performance enhancement was observed with training but not as a function of treatment (online, offline before, or offline after vs. sham). The authors suggest that post-training tDCS promoted memory interference to the detriment of WM performance, and emphasize the importance of stimulation parameters in protocols aimed at cognitive enhancement. Whereas these results do show potentially intriguing changes in cognitive performance within individuals, minimal attention is paid to neural substrates. The authors should reframe this manuscript to meet the focus of the special issue.

Additional feedback:

The hypothesis was that previous enhancement effects following online tDCS would be replicated, but they were not. The reader is left wondering about this from the abstract until the discussion, when it is introduced that the stimulation site differed from the previous study (p. 19). This should be considered especially pertinent given the focus of the special issue.

**Good point. We now include a footnote in the introduction (p. 7) that makes our choice of stimulation site explicit, and reference readers to the Methods (p. 11) where we now explain this choice in greater detail. Additionally, given the focus of the special issue on neural substrates, we further highlight the role of the right DLPFC in promoting memory interference during memory retrieval/reconsolidation and how stimulation of this site may produce the underperformance seen in our results (p. 18-20).**

Online tDCS was expected to "facilitate training performance in a baseline-dependent manner, with greater gains over the weekend compared to consecutive weekdays…" (p. 5). However, participants were matched on both baseline WM performance and starting day so that the weekend fell at a consistent point. These procedures seem to conflict with the hypothesis.

**Thank you for bringing up this point. This is now clarified in the methods to highlight that baseline and starting day were matched between groups in order to ensure comparability between groups. Additionally, it was imprecise for us to say that the weekend fell at a consistent point. What we meant was that the weekend consistently fell either after the 3$^{rd}$ or 4$^{th}$ training day depending whether participants started training on a Tuesday or Wednesday. Thus, comparisons can be made between Tuesday and Wednesday cohorts to tease apart the effects of when the weekend appears. Verbiage in the manuscript has now been amended to make this clear (p. 8-9).**

Why was right DLPFC selected at the stimulation site? Details on study rationale are needed to contextualize this study. This is especially confusing as the abstract indicates a negative role for DLPFC in memory, yet the introduction only indicates previously observed benefits of DLPFC stimulation. It shouldn't be assumed that readers are familiar with previous work by this group. Same goes for the n-back task design (p. 8).

**The right DLPFC was selected due to its role in promoting visuospatial working memory given that the used training task was visuospatial in nature. More details on both the tDCS setup as well as the n-back training task are now added to the Methods section under "*Working Memory Training"* (p. 10) and *"Transcranial Direct Current Stimulation"* (p. 11).**

More details are needed about stimulation parameters. For instance, where were the anode and cathode placed? What was the "shortcut algorithm" used to target DLPFC (p. 9)? What was the sham stimulation procedure?
**More details about the stimulation parameters are now included in the Methods section under "Transcranial Direct Current Stimulation" (p. 11). In short, the anode was placed slightly lateral to position F4 and the cathode was placed over Fp1, the shortcut algorithm is a computational approximation of the standard 10-20 system that requires fewer head measurements, and sham participants received stimulation only during the first and last few seconds.**

Figure S1 shows maximal electric field intensity at bilateral frontal pole, not right DLPFC. This is critical. It is also indicated in the text that this figure does not show the polarity of current flow – why not? Perhaps a more detailed model would reveal current flow from right DLPFC to frontal pole – or should this paper be about the frontal pole? And, given the focus of the special issue, the current modeling figure should not be supplemental.
**Thank you for bringing this up. We have moved the computational model from the supplementary materials to the main text and now describe in the Results under "Current Modeling" (p. 14) that the peak intensity is over the frontal pole rather than the right DLPFC. However, we also point out that it is not known whether the difference in intensity (~.25 V/m vs. ~ .4 V/m) is functionally meaningful. Moreover, upon the reviewer's suggestion, we now include a model of electric potential, which shows polarity. Here, we see that the peak positive potential is around the right DLPFC, whereas the potential over the frontal pole is slightly negative. Thus, we might expect more excitatory effects over the right DLPFC rather than the frontal pole. We also further point out that the effects of tDCS are also task-dependent. So, to the extent that the n-back task activates the right DLPFC more so than the frontal pole, tDCS should also theoretically exert a more meaningful effect over the right DLPFC than the frontal pole. All in all, we believe that our stimulation parameters do indeed target the right DLPFC, and have worked to ensure that our discussion (p. 16-17) surrounding the role of the right DLFPC in tDCS-enhanced WM training provides sufficient justification.**

Analyses showed: (1) a session effect reflecting performance enhancement regardless of tDCS treatment; (2) a session x treatment interaction such that the post-training group improved the least; and (3) the post-training group showed stronger gains after the weekend relative to a weekday. These results do not seem to support a detrimental effect of post-training tDCS, but rather relatively less benefit.
**We have clarified our wording throughout the manuscript to make it clear that any detrimental effects are relative to sham (p. 3, 15, 17). We still contend that the word "detrimental" is appropriate because learning is impaired not only relative to other tDCS conditions, but also relative to not receiving tDCS at all.**

Further, what should readers make of the post-training weekend effect? Should spacing out

training be considered a possible remediation of post-training DLPFC stimulation effects? How might this make sense considering the brain?
**Based on reviewer 2's comments, we have more clearly separated the exploratory *post-hoc* analyses from the main analyses. Since the post-training weekend effect falls under the post-hoc/exploratory category, we also refrain from overinterpreting. However, in our discussion, we now consider that this may represent a washout of the disruptive effects of post-training stimulation, and caution future research to entertain this possibility when spacing out follow-up assessments. (p. 21).**

Given the focus of the special issue, the authors should consider discussing WM performance enhancement in the context of published training studies that included neuroimaging measures.
**We have now incorporated a few studies in the introduction describing both the imaging of tDCS and areas pertinent to WM such as the DLPFC (p. 4). Additionally, we have added more detail throughout the manuscript on the possible neural/brain bases of our effects to fit the special issue (p. 16-20).**

The discussion of left DLPFC stimulation as beneficial and right DLPFC stimulation as detrimental suggests the problem is the right hemisphere. However, there is literature showing that right-hemisphere tDCS improves WM.
**We have made it more clear in the revised manuscript that the disruptive effects of the right DLPFC on memory critically depend on the reactivation and lability of recently learned memories/engrams (p. 20). Thus, based on our findings here, we are only arguing that right DLPFC stimulation *after* WM training is detrimental, which does not contradict the studies showing online benefits of right DLPFC stimulation concurrent with training.**

Finally, the discussion of right DLPFC in memory interference does not necessarily explain the results. Although it is reasonable to consider shared neural mechanisms between WM and longterm
memory, the n-back study-test trials are fully contained within a session. So, what would be interfered with after an n-back session? This differs from a long-term memory experiment with study and test separated by stimulation potentially promoting interference.
**We have now added a literature review in the introduction that discusses the consolidation of WM training skills after a period of sleep or a nap, and discuss the consolidation of these skills as akin to consolidation of procedural skills (p. 5-6). Thus, we argue that is the procedural skills involved in performing the n-back task (mapping between cognitive response and button press, shifting between n-back levels, shifting between different strategies, deciding when to update contents in WM versus rehearsing old information, etc.) that can both be consolidated, as well as interfered with, by tDCS.**

**Reviewer 2**
This controlled trial investigated the effect of timing of tDCS on repeated working memory training. The study attempted to replicate results from a prior study (Au et al., 2016) though was unable to show a significant effect of online relative to sham tDCS in facilitating training performance unlike the prior study. Interestingly, they found instead that offline tDCS administered after training impaired learning and consolidation of training effects. The results are interesting and represent an important addition to the field.
I have though the following concerns/queries regarding the paper in its current form.
MAJOR
1. Statistical analyses. Many post hoc analyses were conducted with unclear rationale. These

further do not significantly contribute to the main results.
**We appreciate your perspective on this point. We have now clarified in the "*Post-Hoc Analyses"* section of the Results that these analyses are exploratory and conducted because the detrimental effects of post-training stimulation on working memory training is a novel finding, and we felt it was important to fully characterize its effects on n-back training (p. 15). We believe these post-hoc results do contribute to the main results, because they reveal a few important observations that would otherwise be missed: 1) they allow us to characterize relative efficacy of the post-training condition to the other two stimulation conditions, rather than just to sham, 2) they reveal persistent underperformance present at the 1-month follow-up and 3) they demonstrate the effect of the weekend on training performance, which was null in the overall analysis. However, we make a clearer effort to center our discussion around the main results, and pay special attention to label these exploratory post-hoc results as such when discussing them (p. 20).**

2. Statistical analyses. Table 1. It is not clear why the models were repeated using the offline post training condition as the reference. This does not add anything to the main results presented in the Table with the sham reference (which was consistent with the addressing the stated hypothesis).
**As elaborated in the comment above, the post-training reference was used in order to more fully explore our novel finding, and we believe it does add a few nuances that are not present in the main results, as numerated above. However, we emphasize the exploratory nature of these results and base the bulk of our discussion on our main results.**

MINOR
1. The Introduction would benefit from citing other relevant papers in this field (Jones et al., 2015, Plos One, Martin et al., 2013, Int J Neuropsychopharm, Richmond et al., 2014, J Cog Neurosci).
**These citations have been added.**
2. Methods. Was a power analysis conducted? If so, this should be described as effects from tDCS are typically small.
**A power analysis is now described in the Results under "Demographics and Sample Size" (p. 13)**
3. Please describe the stimulation montage and parameters in more detail, including sham.
**These details have been added to the Methods (p. 11).**
4. Results. Please describe the demographics of the 3 groups, i.e., age, gender, education. These factors can potentially affect outcomes.
**This information has been added under "Demographics and Sample Size" (p. 13). Information on education is not included because our sample consisted entirely of undergraduates who all have roughly the same level of education. Furthermore, age (which is now included) can also be used as a rough proxy for their level of education.**
5. As the main effect of condition for the ANOVA analysing the 1 month effects was nonsignificant,
this indicates that there was no difference between conditions. The results should be interpreted as such in the Discussion, i.e., page 16.
**We now clearly separate the main results from the *post-hoc* exploratory results. We only briefly discuss the 1-month effect in the context of our *post-hoc* results and are very explicit about labeling it as an exploratory effect (p. 20).**

JOURNAL OF Neuroscience Research

2nd Editorial Decision

Decision Letter

Dear Dr Jaeggi:

Thank you for submitting your manuscript to the Journal of Neuroscience Research. We have now received the reviewer feedback and have appended those reviews below. I am glad to say that the reviewers are overall very enthusiastic and supportive of the study. They did raise some concerns and made some suggestions for clarification, but I expect that these points should be relatively straightforward to address. If there are any questions or points that are problematic, please feel free to contact me. I am glad to discuss.

We ask that you return your manuscript within 30 days. Please explain in your cover letter how you have changed the present version and submit a point by point response to the editors' and reviewers' comments. If you require longer than 30 days to make the revisions, please contact Dr Junie Warrington (jpwarrington@umc.edu). To submit your revised manuscript: Log in by clicking on the link below

(If the above link space is blank, it is because you submitted your original manuscript through our old submission site. Therefore, to return your revision, please go to our new submission site here (submission.wiley.com/jnr) and submit your revision as a new manuscript; answer yes to the question "Are you returning a revision for a manuscript originally submitted to our former submission site (ScholarOne Manuscripts)? If you indicate yes, please enter your original manuscript's Manuscript ID number in the space below" and including your original submission's Manuscript ID number (jnr-2020-Jul-8890.R1) where indicated. This will help us to link your revision to your original submission.)

Thank you again for your submission to the Journal of Neuroscience Research; we look forward to reading your revised manuscript.

Best Wishes,

Dr Elizabeth Johnson
Associate Editor, Journal of Neuroscience Research

Dr Junie Warrington
Editor-in-Chief, Journal of Neuroscience Research

Associate Editor: Johnson, Elizabeth
Comments to the Author:
Dear Dr. Au,

The reviewers were enthusiastic about your revised submission to the special issue on the neural substrates of cognitive change in humans. Reviewer 1 noted a few remaining issues, which we hope you will address with minor revision.

Thank you for your submission,
Drs. Elizabeth Johnson and Kevin Jones

Reviewer: 1

Comments to the Author
Thank you for a nice revision, which more explicitly focuses on the relationship between brain and behavior and qualifies how tDCS might interfere with WM consolidation processes post-training. I have a few remaining comments:

The Significance section incorrectly states that none of the stimulation conditions demonstrated learning benefits. All conditions demonstrated learning benefits, but no active conditions were beneficial vs. sham.

There is evidence that pairing tDCS with WM training affects WM performance and associated EEG measures 24 hours after training (Jones et al., 2017, 2020). Findings support claims that tDCS affects task-dependent neural activity.

The additions to Figure 2 are useful, indicating that maximum tDCS potential is proximal to the DLPFC anode site even if peak intensity is frontopolar. However, a more informative description of this is needed in the legend and Results for readers to understand the difference between parts B and C, and relationship

between brain and behavior in this study.

It may also be acknowledged as a limitation/future direction that modeling used a template brain but would benefit from an individualized approach using participants' MRI scans.

Reviewer: 2

Comments to the Author
The authors have satisfactorily addressed my prior comments.

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Authors' Response

Dear Editor,

We are delighted to hear that the reviewers were pleased with our latest revision, and are excited to move forward with the submission process at the Journal of Neuroscience Research. Changes are highlighted in yellow in our revised manuscript, and we have included in **bold** below a point-by-point response to the remaining reviewer concerns:

Reviewer 1:

The Significance section incorrectly states that none of the stimulation conditions demonstrated learning benefits. All conditions demonstrated learning benefits, but no active conditions were beneficial vs. sham.

**Thank you for catching this detail. We have amended the Significance section to clarify that the lack of learning benefits are relative to sham.**

There is evidence that pairing tDCS with WM training affects WM performance and associated EEG measures 24 hours after training (Jones et al., 2017, 2020). Findings support claims that tDCS affects task-dependent neural activity.

**We have now cited these papers in our introduction to clarify to readers that there is empirical evidence tDCS can affect task-dependent neural activity with working memory training.**

The additions to Figure 2 are useful, indicating that maximum tDCS potential is proximal to the DLPFC anode site even if peak intensity is frontopolar. However, a more informative description of this is needed in the legend and Results for readers to understand the difference between parts B and C, and relationship between brain and behavior in this study.

**We are pleased the reviewer finds our changes to Figure 2 useful. We have now further clarified in the legend of Figure 2 and in the Discussion (p. 17) that the maximum electric potential is proximal to the right DLPFC, and that this can increase the transmembrane potential of underlying neurons in order to increase neural excitability.**

It may also be acknowledged as a limitation/future direction that modeling used a

template brain but would benefit from an individualized approach using participants' MRI scans.

**We now discuss this in the limitations section.**

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3rd Editorial Decision

Dear Dr Jaeggi:

Thank you for submitting your manuscript "Post-training Stimulation of the Right Dorsolateral Prefrontal Cortex Impairs Working Memory Training Performance" by Au, Jacky; Katz, Benjamin; Moon, Austin; Talati, Sheebani; Abagis, Tessa; Jonides, John; Jaeggi, Susanne.

You will be pleased to know that your manuscript has been accepted for publication. Thank you for submitting this excellent work to our journal.

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Congratulations on your results, and thank you for choosing the Journal of Neuroscience Research for publishing your work. I hope you will consider us for the publication of your future manuscripts.

Sincerely,

Dr Elizabeth Johnson
Associate Editor, Journal of Neuroscience Research

Dr Junie Warrington
Editor-in-Chief, Journal of Neuroscience Research

Associate Editor: Johnson, Elizabeth
Comments to the Author:
(There are no comments.)

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