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Peer-review report of

Rodriguez Larios, J., & Haegens, S. (2023). Genuine beta bursts in human working memory: controlling for the influence of lower-frequency rhythms. *advances.in/psychology, 1*(1), 1-15. https://doi.org/10.56296/aip00006

Round 1

Dear Authors,

I want to extend my sincere appreciation for your interesting submission to advances.in/psychology.

Two expert reviewers in the EEG field have generously provided detailed, constructive feedback on your manuscript. As an editor, I am genuinely grateful for their invaluable insight and dedication, which reflect the level of peer review quality we strive for at this journal. It is an embodiment of our ethos to value the efforts and dedication of our colleagues by compensating them for their review work, thus fostering a more equitable academic publishing model.

The reviewers have shown great appreciation for your novel approach to assessing beta oscillations during working memory tasks and the potential implications of your findings. At the same time, they have pointed out specific areas for improvement and provided some technical considerations and robustness checks that would enhance the manuscript's clarity and completeness. I will not reiterate their points as they have presented them thoroughly and concisely in their reviews.

In addition to their feedback, I kindly ask you to address the following points:

1. When graphically feasible, please provide exact p-values in figures (e.g., for the bar chart comparisons). For the topographical distributions, is it possible to use different symbols for different p-value cutoffs? Also, please describe what statistical estimates boxplots and ribbons (e.g., panels E, 95% CIs?) represent in the notes of the figures. This varies between articles, making this information important.

2. Please provide a power rationale justifying the sample size in the methods section.

3. Please provide a conflict of interest statement at the end of the manuscript.

4. As one reviewer points out, please ensure the code and anonymized data are available in the OSF repository.

5. In some parts of the text, you refer to the "memory manipulation" as well as "manipulation condition." Please clarify the latter by giving it a distinct label to prevent misunderstandings.

6. There are a few typos in the stats. Also, please avoid brackets within brackets. For instance, instead of "…while memory manipulation only affected response times when load was high (lower reaction time in Load 3 Switch relative to Load 3 Stay) ($t(26) = -13.29$; $p_{bonf} < .001$)(see Figure 1B right panel)." Please write

"…while memory manipulation only affected response times when load was high (lower reaction time in Load 3 Switch relative to Load 3 Stay), $t(26) = -13.29$; $p_{bonf} < .001$; see Figure 1B right panel."

As you refine your manuscript, kindly ensure that you have addressed these and all the queries raised by the reviewers. Please provide an itemized overview of how you addressed each point including excerpts that demonstrate the changes in your revision letter.

Thank you for considering advances.in/psychology as a platform to share your research. I look forward to receiving your revised manuscript.

Best regards,

Jonas R. Kunst

Editor-in-Chief

Reviewer 1:

The present manuscript analysis event-related beta oscillations with an algorithm in four different parameters. These parameters are i) amplitude ii) duration iii) rate iv) frequency. In general, this analysis could present more general dynamics of beta oscillations during working memory paradigms. Therefore the manuscript has intriguing data and merit publication. However, there are also some points that must be revised before the publication. One of the fundamental problems is the definition of the frequency; the authors do not just analyze beta but also gamma oscillations. 30-40 Hz frequency in general EEG literature was always gamma. Many working memory studies analyzed gamma oscillations in 30-40 Hz range, so this could cause misinterpretation.

1.The authors define beta as "15–40 Hz"; however, the frequency band between 30-40 Hz is mostly defined as gamma. Since many authors use different frequency limits, the beta and gamma frequencies may change according to the authors. However, there is a very general consensus that the frequencies above 30 Hz are gamma.

2.The authors mention that "Modulations in the beta range have received considerably less attention" and "role of beta in working memory is hard to interpret". However, this is not the case; many studies in the literature showed the role of beta responses in working memory. Beta responses are known for their role in somotosensory paradigms, however, the studies performed in the last ten years showed their role on attention, working memory, and emotional paradigms. So this literature was not considered in the recent manuscript.

3.The legends of the figures help us understand the figures. However, it will be nice to include more information in the texts.

4.The authors performed a very detailed analysis of beta oscillations in four different parameters i) amplitude ii) duration iii) rate iv) frequency. These are essential parameters and increase the quality of the paper. However, there are very basic dynamics that should be kept in mind. Beta and gamma responses increase in power, especially during the sensory and working memory paradigms in the first 200 milliseconds. The phase locking of beta and gamma increases during working memory paradigms in the first 200 milliseconds. With the methodology, the authors apply this very basic and very important dynamics are missing. Time is a crucial factor in analyzing beta and gamma oscillations. This is missing in this research. The paradigm includes attention perception and working memory, which will be represented in the first 200-300 millisecond and then button press where the beta event-related desynchronization will be expected. So in the search for the beta dynamics in a paradigm like this, “time” is a crucial factor.

5.The other important factor is the topology; with this methodology and statistics, it is also hard to see the topological differences. Since this is a visual working memory paradigm, parietal and occipital locations should be active in the first 200-300 milliseconds.

Reviewer 2:

Summary

The authors of this study aim to determine whether actual beta oscillations are functionally involved in working memory (WM) processes. Although this question has been investigated previously, the authors novelly apply a method to detect beta oscillatory events (i.e., bursts) that cannot be attributed to artifacts introduced by harmonics of lower frequency rhythms. Basically, the algorithm first detects bursts of oscillatory activity that exceed an amplitude threshold defined at the level of background aperiodic activity and have a minimum duration of a wave period. Then, it selects only those beta bursts that contain the most prominent peak of the thresholded spectrum. In other words, this procedure discards beta bursts that co-occur with lower frequency bursts of higher power. Subsequent EEG analyses are focused on the selected beta bursts by measuring different parameters: amplitude, duration, frequency, and rate. The general finding is that WM retention decreases amplitude and duration of beta bursts, while increasing their peak frequency. Similar effects are found when increasing WM load or with WM manipulation; however, these operations additionally increase the rate of beta bursts. Interestingly, participants with higher beta burst rates during WM retention -regardless of the load or manipulation- are slower to respond. Following a series of studies, the authors frame the decreases in beta amplitude and increases in beta frequency with WM load and WM manipulation as reflecting increases of cortical excitability in task-relevant areas. They additionally hypothesize that decreases in duration and increases in rate of beta bursts during WM retention and WM manipulation are indicators of the transient reactivation of content-specific neural circuits.

General comments

The manuscript is well written and organized. The findings in the main text and figures are communicated very clearly and to the point. The main question of this study was necessary, and the authors approached the question in an original and methodologically simple way. The results of this study will have an impact in future research investigating the role of beta activity in a wide range of brain processes, as it will encourage applications of this algorithm -or improved versions- to control for potential artifacts caused by harmonics of lower frequency rhythms or other sources.

Nevertheless, I see the necessity of a revision of the manuscript on the points listed below. Firstly, there is some important methodological information missing that should be included in the manuscript. Secondly, there are a couple of details in the figures that should be clarified. Thirdly, some control statistical analyses may be necessary. The main weakness I see is that the current findings are not contrasted with additional analysis performed in lower frequency bands. It has not been evaluated whether possible modulations in lower frequency bands by the experimental conditions may have biased the detection of beta bursts, and possibly also the measured parameters included in the statistics.

Specific comments

1. The sample size for behaviour and EEG analyses should be clearly stated in the methods section (and corrected in abstract, if necessary). It is initially reported a sample size of 31 participants in the abstract and beginning of methods section (page 3). However, it is later said that 4 participants were excluded from the EEG analyses, and behavioural statistics were reported over 27 (31-4?) participants. It seems that both behavioural and EEG analyses only included data from 27 participants. If so, please correct the corresponding information in the abstract and explicitly state the final sample size in the methods.

2. The colour code of Figure 1A should be corrected and/or clarified. It is not clear what is the meaning of the colour of numbers 4 and 8 in the right panel of Figure 1A (page 4). I understand that number 4 is the correct answer for the instruction ‘Switch’ in the given example. However, ‘Switch’ is written in green and 4 is in red. Likewise, the instruction ‘Stay’ is written in red but the correct answer 8 is written in green. It might be that the colours represent something else. To avoid misunderstandings, I advise to include a legend or a clarification in the caption of the figure.

3. Time window and other parameters used for time-frequency analyses should be provided (page 5). I assume that the time-frequency analyses were performed within the 3-s windows of fixation and delays. This information should be clearly stated in the corresponding method section, as well as the strategy to prevent edge-artifacts (type of padding, length of the padded time-window before cutting off the edges). To ensure reproducibility, I recommend to also include the resolution in time and frequency domains, as well as the frequency smoothing -where these parameters constant or they changed with the frequency?

4. The frequency range used to detect the beta bursts should be explicitly indicated in the methods (page 5). The definition of beta frequency band is not clearly stated in the introduction and methods. A frequency range of 15-40 Hz is

included in the abstract, but no further definition is provided afterwards. I see critical to include this information clearly in the manuscript and to justify the choice of the beta band boundaries. This aspect is relevant to ensure reproducibility and for appropriate interpretation of the results. Please, discuss why the analyses have not been focused on a more common definition of beta band (e.g., 13-30 Hz). Also, consider the implications if the range 30-40 Hz may have included slow-gamma activity. Curiously, the frequency range represented in first topographic map of Figure 2C is 20-25 Hz. Does this represent the actual range of the mean peak frequency of beta bursts for the sample across the three 3-s periods (fixation and delays)?

5. No scripts or data are openly available. The link to access the Matlab code of the beta burst detection algorithm is included in the methods section of the manuscript (page 5) and is functional; however, it leads to an empty folder. Given that the main addition of the study to the knowledge of the field results from the application of this algorithm, I recommend that the code is made accessible also for the reviewers (when possible).

6. Pearson correlations may be corrected for the presence of outliers. Since Pearson's correlation is sensitive to outliers, I recommend applying some method that minimizes this issue like, for example, robust correlation (when possible).

7. Correlations between mean reaction times and beta burst rates should be computed over an equal number of trials for all conditions. Reaction times and beta burst rates were both modulated by WM load and WM manipulation conditions. When computing the correlations between reaction times and burst rates, it is not said whether the mean values of these measures were computed over an equal number of trials for all conditions within participants. I see important that the composition of trials, especially of the 'Load 3-Switch' condition is constant across participants. Otherwise, the correlations between these measures may not reflect interindividual differences, but rather, random differences in the proportion of trials associated with slower response times and higher burst rates. Related to this, as a possible control analysis, I suggest performing a correlation within each condition.

8. The approach to compute the relative amplitude (z-score) of the spectra in Figures 3-5 E should be explained. For the sake of clarity and as a good practice, I suggest including a brief explanation of how the z-score was computed. Over which data were the mean and standard deviation computed for the normalization of the amplitudes of the spectra? Aperiodic activity, all the data, etc…?

Control analyses to discard that findings are not due to changes in lower frequency activity may be missing. The algorithm discards those beta bursts that co-occur with more prominent lower-frequency activity. This does not mean that some of the discarded beta bursts are not genuine.It is still possible that experimental modulations of lower-frequency activity are determining or biasing the outcome of the beta burst detection. For example, an increase in theta or alpha bands with WM retention, load or manipulation could reduce the number of selected beta bursts over which the parameters are computed. The impact on beta burst rates is straightforward, but this may also affect other parameters. I see the necessity of evaluating whether lower frequency activity (co-occurring with discarded beta bursts) is also modulated by the experimental conditions over the same sensors of the reported beta burst effects. If so, could these changes explain or influence some of the findings in ‘genuine’ beta activity?

Round 2

Dear Authors,

As you can see, both Reviewers were satisfied with your changes. I agree with their evaluation and am happy to accept it for publication in advances.in/psychology.

Congratulations on a great contribution to the field!

Best,

Jonas R. Kunst

Editor-in-Chief

Reviewer 1:

The authors addressed all points adequately, performed the necessary new analysis, and presented the results. The introduction is also revised. The manuscript now merit publication. I do not have any further comments.

Reviewer 2:

The authors have satisfactorily addressed all my previous comments in their replies and revised version of the manuscript. I have no further comments.