

Review of: "The Cocktail Party Effect in Post-Stroke Fatigue: an EEG study"

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Potential competing interests: The author(s) declared that no potential competing interests exist.

The manuscript is very interesting and addresses the presence of behavioral and electroencephalographic (particularly, ERP) modulations of divided attention in a population of post-stroke patients, with different levels of fatigue (high vs. low). To this aim, the authors employed a famous but sometime underestimated paradigm: the (active) oddball. The manuscript is clear and well-written. The employed methodology is overall adequate. However, the report suffers from some shortcomings that, in my opinion, should be addressed, as detailed below.

Abstract

- A brief description of the sample and of the way in which it was split into two groups should be added.

Introduction

- The cocktail party effect is indeed quite different from that assessed with the oddball paradigm, although they have common roots in the divided attention characteristics. Please, appropriately differentiate between the two effects and consider changing the title of the manuscript accordingly (if necessary).
- Please, specify that the paradigm was an active oddball (i.e., that it required the response of the participants to the target stimuli).
- Please, better describe what you mean with "trait fatigue". A trait is normally a characteristic of the individual, which is kept stable over time, whereas fatigue cannot be stable over time. I understand the author refer to the fatigue reported over a week period, but still, "trait" is not the best wording to use here.

Method and results

- The main issue that I identified from a methodological perspective is the absence of data driven analyses for the ERP. The author employed a, let's say, "top-down" approach in the identification of the channels and time-windows to explore in their ANOVAs on the EEG data. However, this approach has been frequently indicated as fundamentally erroneous (see for instance Groppe, Urbach, & Kutas, 2011, "Mass univariate analysis of event-related brain potentials/fields I: A critical tutorial review"). I suggest running a set of confirmation analyses using the Brainstorm toolbox (e.g., cluster-based permutation analyses over all the sampling points; see <https://www.mcgill.ca/bic/software/servicessoftwareadvancedimageprocessingtoolsbeast/brainstorm>) to compare the two groups along conditions.
- The duration of the stimuli (standard, target, and novel) should be explicitly declared, since it can affect the latency effects. Moreover, which kind of novel stimuli was employed? Were they all natural sounds? Was there the possibility

to startle, even unwillingly, the participant?

- Participants' hand dominance (also post-stroke, if changed) should be declared. Why the participants had always to respond to the target stimuli using their right hand? Using the dominant hand may have been a better approach.
- Please, declare the mean % of interpolated channels over the total.
- Is the % of removed epochs reported between brackets a mean? Please, add also the range.
- Given the presence of a marginally significant difference between males and females over the fatigue dimension, it would be useful to verify the presence of gender-related difference in the results. I suggest running new analyses both for the behavioral and the ERP data, differentiating not only between high and low fatigue, but also between men and women.
- Please, provide a justification for the ceiling effect of the accuracy results.
- Please, add the topographic maps and ERP plots also for P300b and N100.

Finally, I suggest to tone down the conclusions and the discussion regarding the presence of a clear effect of fatigue from an electroencephalographic perspective unless the confirmation, data-driven, analyses will confirm the presence of the reported effects.