

Culture-Driven Neural Plasticity and Imprints of Body-Movement Pace on
Musical Rhythm Processing

PCI Registered Reports

Recommender's Comments:

Dear Dr. Guérin,

Thank you for the revised version of your RR. As you will see, the reviewers made some important comments that need careful attention to achieve Stage 1 in-principle acceptance. In particular:

Reviewer #1 makes several points, including a critical concern about the potentially overestimated effect sizes. This could affect the statistical power, error rate, and precision of your results. This point must be addressed for the study to have reliable results, especially considering that, as noted in your response, “registered reports necessitate a careful justification of the required sample size for each research hypothesis”.

Reviewer #2, Dr. Anne Keitel, remains concerned that the study might measure an auditory effect rather than a movement effect. However, considering time limitations, she is okay with highlighting this conceptual issue in the discussion sections.

Please carefully consider the points raised by the reviewers.

Regarding my own comments, I think keeping mixed-model ANOVAs as the main statistical tests but adding linear mixed models (LMMs) as complementary analyses is a good alternative. However, I want to point out that, while it is true that estimating power for linear mixed models may be difficult without previous data, simulations offer a great alternative (you obviously do not need to do this given that ANOVAs will be the main statistical tests, but I want to mention at least one example included in previous reviews for PCI-RR that could be relevant: the round #2 review by Lisa DeBruine for How does perceptual and contextual information influence the recognition of faces?. The actual review and code for the simulation are available in Rmd and HTML formats here).

I look forward to receiving your revised submission along with a detailed response to the reviewers' comments.

Response: We are grateful for your invitation to submit a revised draft of our Stage 1 manuscript, and for the input that we received from you and the two expert reviewers. Thank you also for sharing this reference and associated code. We have addressed point-by-point each of the reviewers' comments (see below).

Reviewers' Comments:

Reviewer 1 (Anonymous)

The authors have responded to my previous comments and made some changes in response but also rebutted quite a few of my comments, which to some extent of course is their prerogative and I respect their autonomy as researchers to make ultimate decisions. I do have some additional suggestions including a critical one related to the assumed effect sizes that were used to estimate sample sizes needed.

Response: We thank the Reviewer for the time devoted to evaluating our contribution and the insightful feedback that has significantly improved the quality and clarity of our work.

Line 156 “beyond mere detection of acoustic periodicities” This is odd to say when followed by cues that include tempo and rhythm, which have obvious periodicities that are relevant to the metre. Even timbre could be presented with periodic fluctuations (e.g., alternating plucking and bowing of a violin) that emphasize a particular metre.

Response: We thank the Reviewer for raising this potential confusion. The expression “beyond mere detection of acoustic periodicities” to characterise mapping of an internal meter was indeed referring specifically to scenarios as described in the preceding sentence (i.e., rhythmic inputs where “the sensory input lacks unambiguous periodic arrangement of salient acoustic features – as in so-called syncopated (Witek, 2017) or contrametric (Kolinski, 1973) rhythms, where rhythmic and metric structures show a degree of incongruency, which are typical for numerous genres of popular, groove-based music around the world [...]”). But we fully agree with the Reviewer that this does not include all the other cases of rhythms where metre is directly provided by prominent periodic fluctuations of acoustic features cuing particular metric periodicities.

To clarify the specificity of our statement, we have now changed the formulation as follows (see p. 7, l. 155–157):

“In these specific cases, metre perception must rely on internal processes beyond mere detection of acoustic periodicities in the relevant temporal range (Lenc et al., 2021; London, 2012). One of these processes is the learned association between contextual cues (e.g., particular rhythmic figure, timbre, tempo, and social setting) and a specific internal metre (Kaplan et al., 2022; London, 2012; London et al., 2017; van der Weij et al., 2017).”

Line 177 “a more direct effect of movement-related processes on metre” How does this model entail a more direct effect of motor mechanisms compared to predictive-coding and neural resonance theory. They all seem to have similarly direct interactions of sensory and motor processing, and as I pointed out in the last set of reviews and now acknowledged later in this section, it’s not clear how distinct any of these theories really are from each other.

Response: We have amended this sentence following your remark (see p. 7–8, l. 177–180):

“Also suggesting an effect of movement-related processes on metre perception, the active sensing framework states that the motor system modulates the cortical processing of

auditory information by refining attention surrounding relevant sensory information (Morillon et al., 2015; Morillon et al., 2014).”

Line 183 “More radically, the action simulation for auditory prediction” What’s so radical about this theory?

Response: The term “radically” was referring to an aspect of the ASAP hypothesis whereby it is movement planning *per se* that drives meter perception. We have nonetheless removed this adverb to avoid confusion (see p. 8, l. 183–185):

“The action simulation for auditory prediction (ASAP) hypothesis proposes that the simulation of periodic movement shapes metre perception (Patel & Iversen, 2014; Proksch et al., 2020).”

Re: studies by Philips-Silver et al., the descriptions “body movement coordinated with a rhythmic pattern” and “the metre the individual had previously moved to” make it sound like the participants were purposely moving to the rhythms, whereas at least in some of their studies, participants were passively moved by an experimenter, implicating the vestibular system more than the volitional motor system. Please clarify this.

Response: You are right, the reported effects are driven, at least in part, by vestibular-mediated processes. We have added clarification to our statement (see p. 8, l. 197–200):

“For example, both active and passive body movement coordinated with a rhythmic pattern according to a specific metre was found to bias the way individuals subsequently perceive a rhythm, possibly through vestibular-mediated processes (Phillips-Silver & Trainor, 2008; Trainor et al., 2009).”

Line 206 “more direct methods” As I said before, what’s so direct about measuring brain activity and movement? If you mean this is a more direct measure of metre perception, how could they be more direct that actually measuring *perception*? You can see brain activity such as SSEPs even in participants that are not attending or perhaps even asleep so clearly the interpretation of such activity as perception-related is not direct. To be clear, I’m not saying that the proposed measurements aren’t informative, just that they aren’t more direct or “better” than perceptual measures, i.e., again there is no gold standard as the term “direct” implies. So just maybe don’t use the term direct, and instead say that convergent evidence is needed across different kinds of measurements to show how movement can shape metre processing.

Response: Thank you for raising this potential confusion. We have modified this sentence accordingly (see p. 9, l. 205–209):

“Convergent evidence across various forms of measurements (e.g., measurements of both the neural and behavioural responses as recorded in separate sessions in response to rhythmic stimuli) could thus help moving a significant step toward a comprehensive understanding of how movement can shape the internal representation of metre.”

Line 423 The term “statistical learning” is still used once.

Response: Thank you for pointing this out. It has been duly removed (see p. 16, l. 422–427).

“[...] and flexibility to override it is not part of the perceptual learning processes [...]”

Participants: you won't exclude people with psychiatric or neurological impairments such as autism, schizophrenia, ADHD etc. that are associated with sensory-motor and temporal processing abnormalities?

Response: We agree with the Reviewer that it is safer to include participants who do not self-identify as having psychiatric or neurological disorders. We have therefore adjusted the inclusion criteria accordingly, as follows (see p. 16–17, l. 435–439):

“Adult volunteers considered eligible to participate in the study will be aged between 18 and 45 years, non-musicians and non-dancers, free of sensory (i.e., no auditory impairment or uncorrected visual impairment) and motor dysfunctions (i.e., no upper- and/or lower-limb disorders), and not self-identify as having psychiatric or neurological disorders.”

The inclusion criteria were set such as to enhance the generalisability of the results while relying mainly on self-identification of the patients relative to psychiatric or neurological impairments (see Khan et al., 2005).

Line 446. What does “(French and English excluded)” mean? Does that mean they can't speak those languages or it doesn't count as one of the languages from the African countries so they would have to speak English and an indigenous language?

Response: We appreciate you bringing this to our attention. By this expression, we indeed meant that participants would have to speak a local language other than French and/or English (which is the official language in most of our targeted countries, e.g., Côte d'Ivoire, Togo). However, it appears plausible that enculturated individuals, even first-generation immigrants, do not necessarily speak a local language in addition to French (or English). Therefore, to alleviate this potential limitation in our recruitment, this criterion has now been removed from the screening questionnaire, as endorsed by our co-author with extensive expertise in these cultural matters (see p. 17, l. 444–449 and Supplementary File 1, p. 4–5).

For the power analysis, some of the effect sizes used, especially for the interactions, seem very large, e.g., $f=.89$ ($f=.40$ is already considered a large effect according to common heuristics). This seems unrealistic and based on prior experiments that themselves were probably underpowered and therefore led to imprecise estimates of effect size. Specifically, Chemin et al. only used 14 participants in each of the experiments and only Experiment 1 showed a significant interaction effect but not Experiment 2, with very different estimates of effect size in those two experiments, which was glossed over on page 9 when the study was discussed in this proposal. The movement condition manipulation is a type of biasing of perception and there is very good reason to believe these kinds of effects will be quite small based on other studies besides Chemin et al. Specifically, SSEPs in both vision and hearing are mostly stimulus driven with small effects of attention and perception-related effects.

Response: We fully agree with you that in the (non-registered) literature, effect sizes usually gravitate towards high values. It is however important to note that we ran our sample size analysis, for each research hypothesis, using very high statistical thresholds, namely $\alpha = .02$

and $1-\beta = .90$ instead of the usual $\alpha = .05$ and $1-\beta = .80$ (which would have resulted in a final sample approximately half the size, i.e., $n = 12$ per experimental group). Thus, we are confident that our final sample of $n = 20$ participants per experimental group will allow us to test our research hypotheses, even if the effect sizes we used for specific hypotheses are inflated due to being derived from non-registered articles. In addition, the significant p values will be interpreted in a measured, considered manner by comparing the obtained effect sizes with the SESOI.

Reviewer 2 (Anne Keitel)

I would like to thank the authors for their clarifications and thorough responses to my questions.

Most of my comments have been addressed, but I am still worried about the possibility that the authors measure an auditory effect, rather than a movement effect. The senior author's own seminal study on auditory imagery (Nozaradan et al., 2011) provides evidence for the argument that, even without practicing movements, participants can imagine a certain beat during listening, which leads to peaks in the EEG power spectrum. The worst-case scenario is that movement does not add anything, and the measured effect is purely based on participants' auditory exposure to the imposed beat. There might not be a "multisensory" or "holistic" effect (page 10, lines 238,241), or even additive effect of movement, there might simply be none. This is very pessimistic thinking, but it would be good to provide any empirical evidence (or indication) that movement indeed might have an effect on subsequent "shaping of auditory information" within this research context.

I absolutely understand that it is not feasible to double the participant numbers, but a small pilot study would have helped to alleviate the worry that the main aim of the research ("to capture direct neuroscientific evidence for the shaping of auditory information by the pace of previous movement") cannot be addressed unambiguously. However, I don't want to stand in the way of this interesting project to get started, and so I'm okay with the solution that this conceptual issue will be highlighted in the respective discussion sections. All the best for your study!

Response: We would like to express our sincere gratitude for the time you dedicated to reading our contribution and for providing us with your valuable appraisal. We will indeed highlight this limitation of the study in the respective discussion of each Stage 2 manuscripts.

References

Khan, A. Y., Preskorn, S. H., & Baker, B. (2005). Effect of study criteria on recruitment and generalizability of the results. *Journal of Clinical Psychopharmacology*, 25(3), 271–275. <https://doi.org/10.1097/01.jcp.0000161497.73514.80>