Manuscript #646

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

PCI Registered Reports

Recommender's Comments:

Dear Dr. Guérin,

My sincere apologies for the delay in providing feedback on your RR. Securing reviewers proved challenging, especially towards the year's end. However, once suitable individuals were found, their evaluations were detailed and promptly completed. I believe these reviews will greatly assist you in revising this Stage 1 plan.

Although both reviewers provided substantial recommendations for enhancement, their overall assessment of the proposed studies is positive. Reviewer #1 highlights several important concerns, including acknowledging the concept of active sensing, considering recent evidence on metre perception, clarifying the state of knowledge on brain oscillatory interactions, distinguishing between related theories, reconsidering movement and brain activity as the gold standard for measuring beat perception, refining participant exclusion criteria, addressing potential confounds related to metronome use, clarifying technical details, and considering Bayesian ANOVAs to address potential issues with sample size and interpretation of null findings.

Dr. Anne Keitel, the second reviewer, suggests adding a control condition without movement to rule out alternative interpretations, replacing pure tone stimuli with real drum sequences for better ecological validity, providing clearer information on participant samples and stimuli in abstracts and introductions, clarifying inclusion criteria regarding cultural exposure, considering self-report questionnaires for music exposure, and justifying the chosen alpha value of .02.

Response: Thank you for extending an invitation to submit a revised draft of our Stage 1 manuscript. We appreciate the valuable input from both you and the two expert reviewers. Below, we have meticulously addressed point-by-point each recommender's and reviewer's comments and concerns. For ease of reference, we have provided page and line numbers that correspond to the revised manuscript file.

My own reading was mostly concentrated on the RR and statistical analyses because I am not a specialist in the topical area. The primary issues that I would like you to consider are outlined below:

1. Most hypotheses are proposed to be tested by modelling the experiment as either pairwise t-tests or Mixed-model ANOVA. However, this approach, which only models fixed effects, has some limitations. I believe fitting mixed models (Bolker, 2015) that include random effects, such as participants, may be preferable, as it would allow for the generalisation of results to a wider population of participants (Barr et al., 2013; DeBruine & Barr, 2021). Linear mixed models could be fitted with random intercepts per participant, or even random intercepts and random slopes between sessions for

each participant. To implement this in R, you could use packages such as brms (Bürkner, 2017) if you decide to follow Reviewer #1's suggestion of using a Bayesian approach. Alternatively, packages such as lme4 (for general or generalised models; Bates et al., 2015) or lmerTest (only general —normal/Gaussian— models; Kuznetsova et al., 2017) could be used if you decide to maintain a frequentist approach. Regardless of the chosen approach, whether frequentist or Bayesian, post-hoc comparisons can be tested using packages like emmeans (Lenth, 2024).

Response: Thank you for this suggestion. We do agree that the use of linear mixed models would be interesting to account for inter-individual variability. Registered reports necessitate a careful justification of the required sample size for each research hypothesis and estimating power for linear mixed models is difficult when no previous data is available (Kumle et al., 2021). Thus, we decided to keep mixed-model ANOVAs as the main statistical tests, but in line with your recommendation, linear mixed models will be included as additional, complementary analyses in a supplementary file (see p. 28, l. 770–773).

2. On page 27 (line 740) you state that "Normality will be checked using the Shapiro–Wilk test" and that "if violated, the data will be normalised using a transformation that will be contingent on data distribution curves". I want to point out that when fitting general models, such as an ANOVA or a linear mixed model, the assumption of normality pertains to the residuals of the model, not necessarily the dependent variable itself. For this reason, I believe this approach may not be the most appropriate.

Response: We appreciate you bringing this to our attention. This has now been remedied as suggested (see p. 29, l. 783–784).

3. **Minor points**

- As mentioned by Dr. Keitel, I wonder what the rationale was behind setting an alpha of 0.02. To clarify, I am not against this decision, but I believe it should be explained in the text.

Response: We set the significance level at $\alpha = .020$ in accordance with the strictest available stipulations (i.e., from the PCI RR-friendly journal *Cortex*), in order to not restrict ourselves in terms of possible publication venues. It has been clarified within the manuscript (see p. 28, l. 763–765).

- In a few places, you mention that some computations were performed using RStudio (e.g., lines 440 and 724). However, you should cite R instead, as it is the software that performs all computations. RStudio is a very useful IDE that aids in interacting with R, but while all your code can be run in R (regardless of whether you are using RStudio or a different IDE or interface, or even just directly from a command line), your code cannot be executed in RStudio without R.

Response: We totally agree and have thus modified the sentence accordingly (see p. 18, l. 475, p. 28, l. 763).

- On page 18 (line 474), you cite Figure 2, Panel A. However, unlike Fig. 1, Fig. 2 does not include panel labels, so perhaps these are to be added if you want to refer to a specific panel.

Response: Thank you for pointing this out. We made the required correction (see Figure 2, p. 49).

Reviewers' Comments:

Reviewer 1 (Anonymous)

This registered report proposes to test whether movement can influence the recorded brain activity to a subsequent rhythm presented without any concurrent movement. The underlying assumption is that such pre-stimulus movement and the associated beat perception will modulate the brain activity (and clapping) to the rhythm alone and result in beat-related brain activity. Furthermore, an interaction between the movement type and the participants' cultural background will be tested for. I have several suggestions below to improve the paper and the study itself.

Response: We thank you for the time devoted to reading our contribution. We have addressed each of your insightful comments and provide point-by-point responses below.

In the Introduction, the idea of active sensing is introduced. This idea goes back at least to J.J. Gibson and the ecological approach to psychology and should therefore be acknowledged. This includes rhythmic sensing because Gibson talked about how animals locomote (which is rhythmic) and thereby sense their environment below their feet and with their visual system. Furthermore, when you say "the way movement might shape perception is less straightforward", I'm not sure I agree with this because as we move in our environment we hear our own footsteps and people may even use echolocation, which has been studied in various animals, most notably bats of course.

Response: We have duly added the suggested reference to the seminal work of J. J. Gibson (see p. 5, l. 115). In regard to the second comment, we agree with the Reviewer that motor control of sensory inflow has already been substantially investigated in the auditory modality, where echolocation is an obvious example of active auditory sensing. Nonetheless, in species such as humans, who do not use echolocation as a main sensory system, whether such an active sensing process is used to control and facilitate sensory inflow remain unclear (Schroeder et al., 2010). This has now been clarified within the manuscript, as follows (see p. 5–6, l. 119–122):

> "This so-called 'active sensing' process is easily conceivable in the context of vision, somatosensation, or olfaction, where eye, finger, or sniffing movements directly contribute to sensory exploration. In the scope of audition, the way movement might shape perception is less straightforward; this is especially true in species such as humans, who do not use echolocation as a main sensory system, wherein the degree to which such an active sensing process is used to regulate and facilitate sensory inflow, thereby optimising sensitivity to external sounds, remains unclear (Schroeder et al., 2010)."

Line 132 "typically takes the form of a metre" Recent evidence from Nave-Blodgett et al., JEP:General2021a and JEP:HPP2021b shows that the extent Westerners actually perceive metre (rather than just a beat) is possibly over-stated, even for highly trained musicians, and definitely for children. How this is for people in other cultures is still unknown. This passage therefore could be re-phrased as "typically takes the form of a beat and sometimes multiple nested beats, or a metre", perhaps with some acknowledgement for the cultural limitation of our knowledge on metre perception at this point in time. Subsequently, when referring to "metre perception", it might be appropriate sometimes to refer to "beat and metre perception" so as not to presuppose that people are always perceiving the multiple levels of a metre. It might also be useful to distinguish being able to distinguish one meter from another based on perceiving a single beat, as in the case of telling whether music is in 3/4 or 6/8 meter vs. actually perceiving the metre, which requires perceiving multiple beat levels.

Response: We fully agree that, traditionally, distinct terms have been used to denote (a) one periodic level within the perceived metre referred to as the beat, and (b) other slower and faster pulses designated as grouping or subdivisions of this beat (Honing & Bouwer, 2018; Large, 2008). Correspondingly, metre is frequently delineated as a hierarchical structure, with the beat serving as a central temporal reference. In the context of the proposed series of studies, "metre" is used as a comprehensive term with no explicit specification about the number of pulse layers, thus minimising underlying assumptions. In essence, we recognise that the quantity of recurring levels in the perceived meter may vary among individuals, contexts, and cultures – and conceivably, in certain instances, only one periodic level may be perceived, as you suggested. In other words, our approach does not preclude the potential for hierarchical organisation in how metric pulses are internally represented, but instead minimises the number of underlying assumptions. We have added a sentence within the manuscript, which hopefully clarifies this approach (see p. 6, l. 139–142; p. 10, l. 250–252), as follows:

> "This internal representation typically takes the form of a metre, which corresponds to a nested set of felt pulsations that are often periodic (Lenc et al., 2021; London, 2012; Vuust & Witek, 2014; of note, in the current study, "metre" is used as a comprehensive term with no explicit specification about the number of pulse layers, thus minimising underlying assumptions)."

Line 165 "Oscillatory interactions between the auditory and motor areas of the brain would be crucial for metre perception to arise". This implies that there is strong evidence for this, whereas my reading of the literature is that there is only circumstantial evidence. Certainly, motor areas have been shown to be active during rhythm/beat perception tasks and there is functional connectivity evidence, but of course this does not imply actual connectivity or interaction and whether any interaction is indeed oscillatory in nature as the cited model/theory suggests is in need of empirical evidence. So please make it clearer what the state of knowledge is on this topic.

Response: Following your recommendation, we have revised this sentence to make clear that this statement relies on an assumption from the neural resonance theory – hence the use of the expression "would be" in the sentence (see p. 7, l. 174). The revised text reads as follows:

> "Notably, according to this theoretical model, oscillatory interactions between the auditory and motor areas of the brain would be crucial for metre perception to arise (Large et al., 2015; Tichko et al., 2021)."

Line 168 This "active sensing framework" sounds pretty similar to Dynamic attending. Are they the same or can your draw any important distinctions? Also, here you talk about "motor delta oscillations", attention, and presumably there is some auditory processing of those events you mention. But is the motor oscillation really a different thing than the attention or just re-descriptions of each other on brain vs. behavior levels, e.g., see the Premotor theory of attention (https://doi.org/10.1016/B978-012375731-9/50035-5)? As for ASAP, is the possibility that "cortical motor planning regions would thus be entrained" any different than the just mentioned motor oscillations or attention entrainment? These theories are not sounding very distinct from each other, which is not necessarily your fault but this fact should be acknowledged or clearer distinctions between the theories should be explained. You go part of the way there when you say "each of them presupposes a strong role of motor production", but I think the similarities are greater than that, at least as you have described them above.

Response: We fully concur with your remark concerning the similarity among current theoretical models. We have amended the text to acknowledge this aspect (see p. 8, l. 191–193), which now reads as follows:

> "Although these theoretical models of musical rhythm perception diverge in a number of ways (e.g., anatomical substrates, directionality of relationship), they can be viewed as mutually reinforcing (e.g., by describing mechanisms at the brain or at the cognitive level); and importantly, each of them presupposes a strong role of motor production in metre perception."

Line 192 "only constitute an indirect way to capture the internal representation" This seems to imply that movement or movement-related brain activity is the gold standard/most direct way for measuring internal representation of beat and meter. I strongly disagree and would argue that there is no gold standard. In the case of movement, many things affect it besides perceived beat/meter, e.g., various kinematic constraints and prior learning unrelated to beat perception. In the case of brain activity, both auditory and motor activity are mostly driven by physical properties of sound and movement (plus a lot of noise), respectively, rather than being pure measures of perceived beat. One might argue that at least in somewhat musically sophisticated adult humans, the most direct way to measure beat perception is to ask them what the beat is, e.g., name the time signature or use a probe beat as in beat alignment tasks, but I would still not call this anything like a gold standard.

Response: In the proposed set of studies, we will use *both* brain and behavioural measures, hence we do not advocate one index over another. We will also analyse the physical properties of the stimulus, with the aim to examine the relationship among these three signals (i.e., acoustic input, EEG response elicited by the acoustic input, clapping movement to the acoustic input). We concur that there is no gold standard for measuring the internal representation of metre, and believe that crossing indices will allow us to provide a fuller picture of the processes underlying rhythm processing in humans. Following your comment, we have amended the text to make our point clearer (see p. 8–9, l. 205–208), as follows:

> "To date, little work has been done using direct methods (e.g., measurements of both the neural and behavioural responses as recorded in separate sessions in response to rhythmic stimuli) with the aim to capture the internal representation of metre elicited by a rhythm."

Line 303 "12-element rhythms to a three-beat metres" Is there any reason to believe this is a true metre in the sense I described above with listeners perceiving two or more beat levels at the same time? Or is the 12 element rhythm just perceived with 3 or 4 beat beats per rhythmic cycle? If the latter, I would not refer to this as a metre so the psychological phenomenon involved is clear.

Response: As mentioned above (see p. 4 of the present document) and now hopefully clarified within the manuscript (see p. 6, l. 139–142; p. 10, l. 250–252), "metre" is used in the manuscript as a comprehensive term, in order to minimise the number of underlying assumptions.

Line 325 “statistical-learning processes” can you clarify what statistic is being learned? It seems more so that a simple beat structure is being perceived (perhaps learned in some sense) and being carried over for the short term while a rhythm is presented that is not entirely inconsistent with the preceding movement pattern. This bears little resemblance to statistical learning as described in many studies, but perhaps a connection can be made more explicit if you think it’s a useful way to think about it.

Response: In line with your remark, we have changed “statistical-learning processes” for a more general process of “perceptual learning” (see p. 14, l. 356).

Line 403 “free of sensory, cognitive, and motor dysfunctions” This is too vague. Will you exclude people with ADHD, any type of autism spectrum disorder, personality disorder that affects cognition? I would caution against excluding too many disorders because you will end up studying a highly non-representative population. Plus it may be challenging to apply similar exclusion criteria in the two cultural groups. You might want to administer a basic IQ or executive function test that is cross-culturally validated so you can be somewhat convinced that you don’t have any major confounds between group and cognitive function.

Response: We appreciate you bringing this to our attention. We have clarified our inclusion criteria (see p. 16, l. 434–436), as follows:

> “Adult volunteers considered eligible to participate in the study will be aged between 18 and 45 years, non-musicians and non-dancers, and free of sensory (i.e., no auditory impairment or uncorrected visual impairment) and motor dysfunctions (i.e., no upper- and/or lower-limb disorders).”

Line 493 “a metronome-like acoustic pulse will be added to the auditory stimulus and will serve as a cue to the beat from the targeted metre” If I understand correctly, the metronome will tell the participants which beat to move to. But in the subsequent rhythm during which brain activity or movement is recorded, how will you know whether it is the movement per se that is having an influence on subsequent brain activity or movement as opposed to the metronome. We know from the Nave et al., 2022 study you cite earlier than even in the absence of movement, just perceiving the beat one way or another can influence subsequent brain activity.

Response: The aim of this programmatic registered report is to capture direct neuroscientific evidence for the shaping of auditory information by the pace of previous movement. If significant, this effect would thus likely be intrinsically supported by a number of distinct processes, including motor planning, visual, auditory, somatosensory and vestibular cues combined together (Phillips-Silver & Trainor, 2008; Trainor et al., 2009). Movement-related shaping of auditory information was purposely adopted in the current studies (a) for its ecological validity in music and dance contexts, and (b) to increase the likelihood of eliciting an effect in the listening block subsequent to the movement priming, due to the mixture of multisensory effects expected to strengthen carry-over effects. Hence, our objective is *not* to define the necessary and sufficient mechanism for the effect of movement on rhythm perception to take place, but rather to capture the brain processes underlying this holistic effect, while not precluding mental imagery of beat or priming by auditory inputs (as in Nave et al., 2022) that could also significantly shape auditory information. We have added this rationale within the manuscript (p. 9–10, l. 230–243) and adapted our terminology throughout to make this point clear (see e.g., p. 2, l. 32–33, p. 5, l. 105–106, p. 6, l. 123).

In addition, we already have a substantial number of hypotheses to test (i.e., seven; see Table 1, p. 42–47), resulting from the manipulation of a large number of factors (i.e., Group, Session, Movement Condition, Metre Frequency). Should we include a non-movement control condition, it would necessitate inclusion across every cell of the analysis, effectively doubling the total number of participants required – that is already quite considerable (N = 80). Moreover, the inclusion of an additional factor would reduce overall statistical power, resulting in an even larger number of required participants in each cell of the analysis. Thus, although we acknowledge the theoretical interest of the suggestion, the addition of this control group – which deviates from our original research question – proves impractical due to logistical constraints. However, we intend to mention this matter in the discussion of each Stage 2 and propose this interesting combination as a compelling avenue for future investigation.

Line 535 In what sense are the ER2A earphones magnetically shielded? Do you just mean the electronics are not right next to the head, or is the electronic portion somehow shielded by the manufacturer or is there custom shielding?

Response: The electronics of the ER2A earphones are positioned at the level of the participant's clavicle (i.e., not right next to the head). We have removed mention of any 'shielding' within the manuscript (see p. 21, l. 569–571).

It's a little confusing about when exactly they will be doing clapping, tempo change detection, and whole body movement, and how this relates to the experimental design. Please clarify.

Response: Thank you for your remark. We have revised the text to provide clarifications (see p. 21, l. 573–574; p. 21, l. 577–578; p. 22, l. 583–584, p. 22, l. 587). Please note that a diagrammatic representation of the experimental design is also available in Figure 1 (see p. 48).

Line 578 Why no electrodes near the eyes to detect eye blinks and horizontal eye movements?

Response: During pilot tests, we realised that additional electrodes on the participant's face could be disturbing during the whole-body movement session. Moreover, a number of prior studies (e.g., Lenc et al. 2020, 2023), in addition to our pilot data, confirm that ICA can be used successfully to remove eye-blink artifacts with our current electrode montage. Note also that participants will be required to keep their eyes fixated on a marker displayed on the wall during EEG recordings (see p. 19, l. 498–500), drastically limiting horizontal eye movements.

"trials showing excessive artefacts will be rejected" How will you define excessive artefacts? It seems likely that there will be plenty of artefacts in each trial given how long the trials are so wouldn't this result in all trials being rejected?

Response: The frequency-tagging approach diverges from conventional event-related potential studies, which entail a substantial number of very short trials; in this latter case, (a) manual scrutiny of these trials proves arduous and (b) there exists room for discarding numerous trials without significant loss of power. In our scenario, because we will have a small number of very long trials, the decision to reject some of them is delicate and is best accomplished manually, on a case-by-case basis (Barbero et al., 2021; Rekow et al., 2022). Therefore, excessive artefacts will be visually identified based on extensive expertise in our research team with this EEG procedure and analysis of long EEG trials used for frequency domain analysis (see e.g., Lenc et al., 2023, 2020; Nozaradan et al., 2017). Most importantly, the $z_{SNR,EEG}$ index will allow to objectively assess the quality of our EEG data (see p. 27, l. 733–742).

Will any baseline correct be applied before or after filtering of the EEG data? I suppose it might not matter for the frequency-based analysis but for display purpose I'm guessing you still do it.

Response: As you accurately mentioned, the EEG data will be analysed in the frequency domain, rendering baseline correction not mandatory.

Given that a number of interactions are being tested and they can sometimes require pretty large sample sizes, I wonder if Bayesian ANOVAs would be useful instead of frequentist so you can provide evidence in favor of both non-null and null hypotheses? Otherwise, null findings will be ambiguous about whether you were underpowered or actually have favorable evidence for a true null finding.

Response: Following the recommender's comment, we decided to use linear mixed-models as complementary statistical analyses (see p. 28, l. 770–773). We chose to maintain a frequentist approach, given that it can also provide evidence for the true absence of effect (e.g., two-one sided tests procedure; see Lakens et al., 2018). Also note that our required sample sizes were carefully computed under high power conditions (i.e., $\alpha = .020$ and $1\text{-}\beta = .90$; see p. 17–18, l. 455–469).

Reviewer 2 (Anne Keitel)

The registered report by Guerin and colleagues presents an interesting and important set of studies on rhythm processing, and is overall well written and thought-through. The hypotheses and their alternatives, and the rationale and feasibility, are sound. I have two methodological points (one major) and some requests for clarification.

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.

Major point

1) The main question of the work is whether movement affects subsequent rhythm processing. Yet, any positive result (i.e. change of rhythm processing after the movement session) cannot be unambiguously attributed to the movement. Instead, an alternative interpretation would be that simply being exposed to the specific metre (by hearing the metre-specific drum superimposed on the sequence for ~20 minutes) might lead to altered rhythm processing afterwards. To rule out this alternative interpretation, I suggest adding a control condition with no movement (and simply listening to the sequence with the drum cue for the same duration as the movement session is). This would strengthen the paradigm quite dramatically, in my opinion. I think it would be sufficient to add the control condition (and therefore additional participants) to only one sample – perhaps the Western-enculturated sample as they might be easier to recruit.

Response: The aim of this programmatic registered report is to capture direct neuroscientific evidence for the shaping of auditory information by the pace of previous movement. If significant, this effect would thus likely be intrinsically supported by a number of distinct processes, including motor planning, visual, auditory, somatosensory and vestibular cues combined together (Phillips-Silver & Trainor, 2008; Trainor et al., 2009). Movement-related shaping of auditory information was purposely adopted in the current studies (a) for its ecological validity in music and dance contexts, and (b) to increase the likelihood of eliciting an effect in the listening block subsequent to the movement priming, due to the mixture of multisensory effects expected to strengthen carry-over effects. Hence, our objective is *not* to define the necessary and sufficient mechanism for the effect of movement on rhythm perception to take place, but rather to capture the brain processes underlying this holistic effect, while not precluding mental imagery of beat or priming by auditory inputs (as in Nave et al., 2022) that could also significantly shape auditory information. We have added this rationale within the manuscript (p. 9–10, l. 230–243) and adapted our terminology throughout to make this point clear (see e.g., p. 2, l. 32–33, p. 5, l. 105–106, p. 6, l. 123).

In addition, we already have a substantial number of hypotheses to test (i.e., seven; see Table 1, p. 42–47), resulting from the manipulation of a large number of factors (i.e., Group, Session, Movement Condition, Metre Frequency). Should we include a non-movement control condition, it would necessitate inclusion across every cell of the analysis, effectively doubling the total number of participants required – that is already quite considerable (N = 80). Moreover, the inclusion of an additional factor would reduce overall statistical power, resulting in an even larger number of required participants in each cell of the analysis. Thus, although we acknowledge the theoretical interest of the suggestion, the addition of this control group – which deviates from our original research question – proves impractical due to logistical constraints. However, we intend to mention this matter in the discussion of each Stage 2 and propose this interesting combination as a compelling avenue for future investigation.

Other points

1) The second methodological concern is that the used auditory stimuli (pure tone sequences) are quite far from being ecologically valid and might even be a bit unpleasant to listen to (although I could not find the stimuli, maybe they are okay to listen to). Would it be possible to replace the pure tones with real drum sequences? This could be completely equivalent to using pure tones, just using a single drum (like a snare or other tom). Making the stimuli resemble real musical rhythms would improve the ecological validity and perhaps even the compliance of participants. In any case, it would be good to mention the reasons for using pure tones.

Response: Thank you for this suggestion. Regarding possible unpleasantness of pure tone stimuli for the participants, we have accrued over the past few years substantial experience using rhythmic stimuli made up of pure tones (see e.g., Chemin et al., 2014; Lenc et al., 2020, 2023; Nozaradan et al., 2012), and they have never been the subject of complaints or comments on their unpleasantness from participants. Most importantly, given the purposes of the current study (comparison of two samples of participants with distinct cultural backgrounds), the rationale for using pure tones was to present the rhythmic pattern in a decontextualized fashion, thus minimising familiarity interference caused by non-rhythmic contextual cues (e.g., instrument type; see Polak et al., 2018; see p. 19, l. 514–519). In line with your comment, we added the auditory stimuli on our Zenodo repository (https://doi.org/10.5281/zenodo.10221480).

2) The abstracts and introductions mention Stage #2, but I think this is still Stage #1, as no research has taken place?

Response: Thank you for pointing this out. We have changed our terminology to avoid any confusion (see e.g., p. 3, l. 51, p.3, l. 71, p. 5, l. 113, p. 12, l. 315–316).

3) It took me a while to figure out which study would use what type of participant sample and stimuli. The first time this is clearly stated is in the Hypotheses section. Before, it was unclear which study uses a Western sample vs. African sample, what music stimuli would be used, and whether this would differ between studies. Could it be stated in every abstract and introduction what the specific sample will be, and that the same (African) rhythmic pattern will be used in both studies? This would help to avoid confusion.

Response: We have added details in each abstract and introduction in accord with your recommendation (see p. 3, l. 57–59; p. 9, l. 219–222; p. 12, l. 307–308).

4) The first paragraph of the Stage 2 [sic] #2 Introduction is quite redundant with the #1 Introduction, although this might be deliberate.

Response: This is indeed deliberate because of the format (i.e., programmatic registered report), and had been specifically discussed with a member of the managing board.

5) The inclusion criteria are a bit unclear (starting line 407). Could you clarify whether it is sufficient for inclusion if participants themselves have not lived in the respective countries? It sounds like it is sufficient if only the parents have lived in the country for 15 years, which begs the question whether individuals who for example grew up in Western countries have experienced enough exposure to African music to be influenced by its culture (and vice versa).

Response: One of our criteria is indeed that the participant or both their parents have lived, at least for the first 15 years of their lives, in one of a set of African countries. Our rationale lies in the observation – based on experience with these communities of one of the co-authors with specific expertise in anthropology and ethnomusicology – that African immigrant families tend to exhibit strong bonds within the diaspora, which could result from the segregation they endured during colonialism and the heightened racism towards African diasporas.

6) As the inclusion criteria are a bit arbitrary, it would be good to ensure and show that participants have been exposed to the relevant music/metre throughout their lives. Could the extend of exposure to Western and African music be added as a self-report questionnaire?

Response: The self-report questionnaire for musical exposure was already included in the previous iteration of the manuscript (see Supplementary File 1, p. 7). Following your comment, we also added a self-report questionnaire on familiarity with the specific rhythmic pattern that will be used in the study (see Supplementary File 1, p. 8–9).

7) Could you please justify where the alpha value of .2 (lines 721 & 725) comes from? Is this based on experience or a specific recommendation?

Response: We set the significance level at $\alpha = .020$ in accord with the strictest available stipulations (i.e., from the PCI RR-friendly journal *Cortex*), in order to not restrict ourselves in terms of possible publication venues. It has been clarified within the manuscript (see p. 28, l. 763–765).

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