*RESPONSE TO COMMENTS FOR THE MANUSCRIPT TITLED “Uncovering beat deafness: Detecting rhythm disorders with synchronized finger tapping and perceptual timing tasks” SUBMITTED TO JoVE (submission n. 51761R2)*

*Editorial comments:*

*Please address the following as well:*

*1) The protocol steps describing the tasks themselves do not say when the experimenter begins and ends the recording of data. It would be useful to add step indicating the start and end of the trials (i.e. "Click start..." or "End recording when...").*

This information has been added.

*2) The Materials table currently lists only Matlab. Will the other software and functions used be mentioned? Can the availability of the script used to detect anisocrony in 2.1.1 be mentioned as well?*

The Materials table has updated, including all the material and software used. The program needed to run the anisochrony detection task (i.e., Presentation) is now mentioned in the table. Since some of the procedures used to implement the anisochrony detection tasks are making the object of a patent request, they cannot be made available at the present stage. Nevertheless, the description of the materials and of the procedure, as well as the reference to previous articles using the anisochrony detection tasks is in our opinion sufficient to reproduce the task.

*3) The False Alarm rate and Hits rate in steps 2.3.1 and 2.3.2 are defined rather than telling how to calculate them. Please detail how to calculate these.*

FA and Hits rate are simply calculated by counting the occurrence of FAs and Hits in the output log file provided by the software used to run the experiment (Presentation). This has been specified, without referring to the specific software.

*4) How is the z-score calculated in 2.3.3?*

This is now indicated.

*5) Minor grammar note, Short Abstract: "Synchronized finger tapping to the beat of simple and complex auditory stimuli, and detecting rhythmic irregularities allow to uncover rhythm disorders."*

The error has been corrected.

*6) Often reviewers request the addition of a large amount of details or explanations. We realize that, especially in the protocol section, brevity and clarity are important for a JoVE publication and expect the focus to be on providing a framework for the method presented rather than a comprehensive review of the research field. Please address each comment in your rebuttal and note if you choose not to include the requested information in the text and the reasoning behind this decision.*

*For example: Reviewer 2*

Although we agree with most of the comments and requests from the Reviewers, a few of them cannot be properly addressed given the constraints of the JoVE publication format. In addition, they go beyond the focus of the protocol description, which is different from a standard experimental report. Details are provided below for each comment.

*7) Please take this opportunity to thoroughly proofread your manuscript to ensure that there are no spelling or grammar issues. Your JoVE editor will not copy-edit your manuscript and any errors in your submitted revision may be present in the published version.*

The manuscript has been proofread by a native English speaker.

*Reviewers' comments:*

*Reviewer #1:*

*Manuscript Summary:*

*This is a nice description of a method for testing perceptual and motor timing abilities in the general population with the goal of identifying individuals with temporal processing difficulties of one sort or another. The approach is well described and it will be a useful contribution to the field.*

*Major Concerns:*

*No major concerns*

*Minor Concerns:*

*I have some wording suggestions to make things clearer in a few places and have included these in an annotated file. See attachment.*

We wish to thanks the Reviewer for the useful suggestions, most of which have been integrated in the revised version.

*I was a bit confused about whether the authors would intend to make available the actual stimuli and scripts required to perform this data collection and analyses or not. In places, it sounds like they are (eg 'Select the stimulus as indicated in the software interface'. But unless I missed something, there are no links to these materials...*

Snippets of the three stimulus types (with a 600-ms IOI) are now uploaded as additional material. This is now indicated in 2.1.2. However, for the reasons mentioned above, the scripts cannot be made available at this time. However, details regarding the steps implemented in the scripts are indicated in the text and allow their reproduction on a given software platform. To eliminate ambiguities, references to specific scripts were removed from the text and only the software for stimulus presentation, response recording, and data analysis were generally mentioned.

*Additional Comments to Authors:*

*N/A*

*Reviewer #2:*

*Major Concerns:*

*1. Making a case for the task set:*

*The authors need to make a stronger and more detailed argument for the need and the advantages for their set of tasks in the context of existing literature. In the present version, I find the praising of the presented set of tasks in terms of their usefulness a little repetitive and not very well backed up. In my mind, even details like the choice of stimuli and response method should be justified - if the authors are aiming to make this battery "the" future choice and standard test battery. I am sure the arguments are there -just not spelled out- and make a few suggestions here.*

*What strikes me the most is a general lack of elaborate description and comparison with existing work on: the nature of previously reported rhythm-related deficits; tapping and perception studies in normal subjects that may be taken as a departure point -for exploring deficits and to describe the advantages of the present tasks in this exploration; evidence for and against common neural substrates underlying perception and action timing, rhythm and melodic processing, and related dissociative deficits -which are only mentioned by citing one of the authors' previous papers; finally and most importantly, existing sets of tasks of beat perception and synchronization with similar aims would deserve mentioning, although some have only been presented at conference proceedings.*

*Most specifically, I would suggest a more elaborate description in the introduction and to fleshing the discussion of disorders to be addressed, in particular that of "beat deafness". This is currently very limited and in clear contrast with the title. Two particular instances to this point: a) Introduction (line 70): mentioning "beat deafness" in brackets only; which is then followed by an explicit description of synchronization impairment in the presence of intact perception only (i.e. the opposite profile in terms of dissociation). b) Discussion (line 374): "detect individuals with beat deafness / poor synchronization" -without much discussion of beat deafness, rhythm deficits or dysrhythmia (see literature recommendations below).*

*Some useful/relevant literature:*

*Assessment:*

*Fujii, S. & Schlaug, G. The Harvard Beat Assessment Test (H-BAT): a battery for assessing beat perception and production and their dissociation. Frontiers in human neuroscience 7, 771, (2013).*

*Farrugia, N., Benoit, C.-E. & Harding, E. BAASTA: Battery for the Assessment of Auditory Sensorimotor and Timing Abilities in International Conference on Music Perception and Cognition. (eds E. Cambouropuoulos, C.*

*Tsougras, P. Mavromatis, & K. Pastiadis) 292-299.*

*Iversen, J.R, & Patel, A.D. (2008). The Beat Alignment Test (BAT): Surveying beat processing abilities in the general population. In: Proceedings of the 10th International Conference on Music Perception & Cognition (ICMPC10), August 2008, Sapporo, Japan. K. Miyazaki et al. (Eds.), Adelaide: Causal Productions*

*Beat deafness/ dysrhythmia:*

*Launay, J., Grube, M. & Stewart, L. Dysrhythmia: a specific congenital rhythm perception deficit. Frontiers in psychology 5, (2014).*

*Dissociated deficits in rhythm and melody:*

*Foxton, J. M., Nandy, R. K. & Griffiths, T. D. Rhythm deficits in 'tone deafness'. Brain and cognition 62, 24-29, (2006).*

*The standard battery for musical disorders, mostly pitch-based but also including rhythm and meter:*

*Peretz, I., Champod, A. S. & Hyde, K. Varieties of musical disorders. The Montreal Battery of Evaluation of Amusia. Annals of the New York Academy of Sciences 999, 58-75, (2003).*

*Synchronization with increasing complexity:*

*Madison, G. Sensori-motor synchronisation variability decreases as the number of metrical levels in the stimulus signal increases. Acta psychologica, (2013).*

*[I refrained from recommending a list of tapping and perception studies from normal population and on the distinction of motor and perceptual timing.]*

We are grateful to the Reviewer for raising these important points, which we are of course aware of. The lack of information provided in the introduction was mostly driven by the requirements of JoVE to provide a simple context for presenting the method and providing a “user guide” in support to the video recording of the method. However, a more detailed introduction has now been provided, following the suggestion of the Reviewer, thus focussing on rhythmic disorders and beat deafness in particular, as announced in the title.

*2. Confusion between content of present vs. previous study:*

*My other main concern may either be easily alleviated, as I may just be mistaken - or else be a major issue. It did not become clear to me, when reading the manuscript, which data and procedures are actually new and which were taken from the "representative" study by the same authors that most of the figures come from (Sowinski & Dalla Bella, 2013). Double checking the previous paper, it appears to me that most of the content of the present paper has been reported in the previous one already, but I may just have missed or misread something.*

*Similarly, it is a bit difficult to follow in the introduction which research is done here and has been done before. This might in part simply be a problem of the use of English tenses (see also minor comments below). One specific instance is the use of "is" in line 105, which seems to refer to previous work but this can only be inferred from the content of the sentence - after the preceding back and forth between previous work in past tense and the present study in present tense.*

It is true that the representative study is one of the studied that we published in 2013, and obviously the reported data is taken from that study. However, please note that the goal of the publications in JoVE is to provide clear instructions to accompany a visual demonstration of existing and working protocols, not to present new experimental results. For this reason strict “rules” are applied to the preparation of the methodological section. Presenting previously published findings as representative results is thus compatible, and actually encouraged, but JoVE.

*3. Evaluation procedure for individual results missing:*

*The instructions in the protocol are very clear and detailed, easy to follow and "idiot-proof" in the positive sense, starting with the instruction to open the software and finishing with the calculation of measures. However, the final and very important "step" that seems to be missing is the evaluation of individual results. This seems odd and I fear I may have overlooked something, as this is what this protocol is supposed to be about, i.e. the uncovering of individual disorders. I saw no mentioning of how significant deviations in the individual were analysed: maybe by z scores (?), based on mean and standard deviation (given the appropriate distribution?), or maybe based on Bayesian statistics following Crawford's methods (?). In addition to a written description, a formula for the evaluation would be useful -also for the calculation of d' (line 271).*

*One specific detail to this comment: it is unclear what "large enough" means (in the "Note: ..." following 1.3.5).*

We thank the Reviewer for raising this important point. A new step has been added to the protocol to specify the operations to be followed to evaluate individual results against the performance of a normative group or a of a control group. The additional information requested was added.

*4. Choice of tasks:*

*Interestingly, the perceptual tasks tests only 1 instance of deviation -as opposed to the sense of the beat across a sequence, which would be more akin to the synchronization tasks.*

*Furthermore, for the motor tasks, it might have been useful to include a synchronization continuation on top of a free-tapping task.*

*Finally, the perceptual tasks surprisingly have no noise condition. This takes some double checking -after several mentionings of the "same material" being used for motor and perceptual tasks and a semi-clear description at the end of the intro. (Minor formal detail that contributes to this point: The description at the end of intro includes some misleading grammar inconsistencies between the motor and perceptual tasks.)*

*As already mentioned in the context of comment 1, I recommend for the authors to discuss the choice of tasks more.*

The choice of the tasks has been better justified in the Introduction of the revised manuscript. We agree with the Reviewer that additional tasks could be added to obtain a more detailed profile of participants’ timing abilities. Note that this suggestion has already be implemented in a battery of perceptual and sensorimotor tasks we have developed (the Battery for the Assessment of Auditory Sensorimotor and Timing Abilities - BAASTA; Benoit et al., in press), which includes a synchronization task. We have acknowledged in the Introduction that there are batteries including more than the tasks presented; at the same time, the purpose of this video article is to illustrate in detail two particular tasks (implemented in more extensive batteries, such as the BAASTA), as they have shown as particularly sensitive to individual differences and to beat deafness. The illustration of these two tasks is also compatible with the time constraints to show the various steps of the procedure in the visual article.

*6. Under-presentation of the "individual differences":*

*I was a bit surprised to see Figures 1 to 4 present group plots, on top of that without any statistical evaluation of the group differences described. Solely Figure 5 shows individual data, for synchronization; no individual perception data are plotted at all. The table on individual results lists only the significant differences to controls, again like the protocol without any mentioning of how these are evaluated. A description of evaluating "disorders" ought to be included in the protocol -representing the main point that the paper is claiming to address. Editorial Note: Please mention any description in the Discussion rather than the Protocol.*

As mentioned before, the text accompanying a JoVE visual article is supposed to include representative data, rather than a full-fledged “Results” section from a regular research article (from JoVE Author Instruction guidelines: “Please add a concise, written description of a “representative” outcome following the use of this protocol, so that a viewer will have a sense of what a “good” or “bad” result looks like.”). Thus, as far as it can be seen in previously published JoVE visual articles, statistical tests are not included in the “Representative results” section of the JoVE manuscript accompanying the visual article. Yet, these analyses are included in the original manuscript where the results are reported in detail (Sowinski & Dalla Bella, 2013).

Following JoVE requirements, the goal of the reported figures is to show what are the general results which are typically obtained in the 2 sensorimotor and rhythm perception tasks presented. This goal is achieved by presenting Figure 1 through 4. In addition, Table 1 was aimed at showing the results of the analysis of individual differences, which has now been described more thoroughly in the text.

The main goal of Figure 5 was merely to show that participants are in general more variable in terms of consistency than in terms of accuracy, thereby leading to choose consistency as the main performance variable for detecting beat-deaf individuals. However, the analysis of individual differences was done in Table 1. More details are now provided which describe the various steps needed to characterize beat-deaf individuals in Table 1, in keeping with the goals of the method. Thus, Figure 5 became unnecessary to present the method, and eventually has been removed. Still, information about the choice of synchronization consistency in the paper for detecting beat-deafness has been underscored, with reference to Sowinski & Dalla Bella (2013).

*5. Appropriate controls and cut-offs:*

*Such a task set/ test battery should in my mind, like the MBEA, ideally come with cut-offs and description of how they were derived, in the best case for different age-groups if needed (or the demonstration of age-independency).*

In the revised version of the manuscript, the procedure used to identify sensorimotor or rhythm perception disorders relative to a normative or a control group is provided. In the representative study, beat-deaf individuals were defined relative to a control group of 23 participants. The cut-off scores relative to this group are now provided.

*Minor Concerns:*

*1. I find the use of "general population" somewhat confusing. Would "special populations" or individuals with profiles of disorder be part of the general population or not? At some points it seems they are, e.g. its first mentioning in the short abstract, as well as at the end of second paragraph of introduction, but at other points, e.g. in the last paragraph of the discussion or the end of the abstract it seems they are not. In other work I seem to recall this term is often used exclusive of those with defined disorders but including those with certain "traits" (weaker than the disorder). The authors might want to be more explicit about this to save the reader wondering.*

By “general population” we indicated here the majority, namely individuals who did not receive musical training. This is now indicated to avoid confusion.

*2. Order of tests: Is this fixed and would the authors expect any order effects, especially with using the same stimuli for action and perception? Would they advise against carrying out perception first?*

The order of the tests is typically counterbalanced across subjects. This is now specified in the representative results.

*3. Synchronization data analysis: 1.3.4 and 1.3.5 seem to be two parts of computing one statistic and should be one sub-point.*

The subdivision of the analysis into 2 sub-steps was requested by the Editor, to limit the number of operations included in each individual step.

*4. The use of "for example" (line 276) when referring to the previous study seems inappropriate; the whole "representative results" section builds on that one study.*

The expression was removed.

*5. A few puzzling wordings in the results:*

*"less consistent" (line 292) - than what (controls or tones)?*

*"asynchrony smaller with more complex stimuli" (297) - does that refer to noise and music?*

*I note in fact in the related figure, that poor synchronizers do anticipate the beat in their tap in noise, which does not become clear in text.*

*"These tasks" (line 303) is unclear (motor and/or perceptual?), as is the content of the statement in this sentence, referring to "individual differences" after looking at group data only.*

*The "In spite of" (line 306) seems to suggest a contradiction - however if this is about individual differences it shouldn't be one. Same is true for use of "even" in following sentence.*

The sentences have been corrected or reworded.

*6. English: there are several instances of odd use of English language, e.g. participants being "submitted" (line 281). I will refrain from making a list of instances that stood out to me, but strongly suggest the manuscript be thoroughly proof-read by a native speaker.*

The manuscript has been proofread by a native English speaker.

*7. Minor formal details:*

*- unclear use of "the same" in the stimulus description in line 245 - which seem to be "the same" \*except 8 beats\* in length rather than 90?*

*- "Precision" = consistency? (in synchronization tasks protocol; Data analysis 1.3.3)*

*- "Representative Results", second paragraph: Consistency and accuracy would for (this) reader better be described in the same order as they are in the protocol and presented in the figures.*

*- Figure 5 could be better presented and labelled (lines etc like in corresponding group figure).*

*- Lim et al. 2005 reference: not formatted in text (and not appearing in reference list).*

*- Table 1 (caption): suggest to change to "correctly perceive a single deviation from the beat".*

*- Line 412 (Discussion): the authors might want to start the sentence with "successfully extended" with something like "We expect that ...".*

The changes have been applied.

*Additional Comments to Authors:*

*N/A*

*Reviewer #3:*

*Manuscript Summary:*

*This paper in an invited method paper describing how to administer tests for measuring both perceptual-motor timing using a synchronized finger tapping paradigm and perceptual timing using a perceptual judgement paradigm. The paper introduces reasons for why one would want to do these kind of experiments.*

*It then describes a specific methodology with representative results from a previous experiment. Finally there is a discussion highlighting advantages of this methodological approach and suggesting future applications.*

*The technical details of how to administer the tests using a digital MIDI percussion instrument and the instructions regarding use of circular statistics for the synchronization task (using CircStat toolbox in Matlab) are straightforward and very useful instructions to those experimenters who are new to this kind of work and who do not yet have technical or analytical abilities in this area of research. The calculation of a discriminability index for the judgement task is also explained and could perhaps be useful (see later comments). Although I have not used the MIDI instrument myself there is not reason to suspect that these instructions will not work.*

*Enthusiasm for the paper in general is tempered by the explanation for the rationale for doing this work, the representation of the results and following discussion. The following are more specific comments.*

*1. Line 49. The use of the term "perceptual and motor timing abilities" is not strictly true. While perceptual abilities can be assessed separately, motor abilities are never tested without perception involved. Even if one asked a person to self-generate their own isochronous tapping and withdrew vision and audition, they would still have proprioceptive input. Thus, the term perceptual-motor task is more appropriate than motor task for the synchronization task throughout the document.*

We agree with the Reviewer, and therefore replaced “motor timing” with “sensorimotor timing” in the manuscript.

*2. Line 51. Minor language issue is "allow to uncover". It should be either "allow one to uncover" or "allow the uncovering of".*

The sentence has been rephrased.

*3. Line 78-82. Is judging the duration of time to a colliding event (and sometimes including movement) the same as predicting a time period based on past memory and are either of these the same as perceiving the timing of a beat and is this the same as being able to time a movement to a beat. To me, these are all separate abilities and should not be presented as though they are all part of a similar problem.*

We concur with the Reviewer that the mentioned abilities are likely to be underpinned by different processes and mechanisms. The reference to phenomena requiring temporal prediction may be misleading here and has been removed.

*4. Line 82. It is not quite clear how the references apply to the statement here.*

The references indicated well apply to the statement, by showing that the majority of individuals without musical training can move along to the beat, and have intact rhythm perception.

*5. Line 85. Only the first reference refers to data that directly verify this statement. There are many better references e.g., Grahn JAW, S.L. Perspectives on rhythm processing on motor regions of the brain. Music Therapy Perspectives Special Issue: Music and Neuroscience Clinical Implications for Sensorimotor Functioning 2013;31:25-30.*

We disagree with the Reviewer, since the three mentioned references indicate, at different degrees, the subcortical and cortical contributions to timing, and thus support the general statement we made. However, we thank the Reviewer for proposing an additional reference which we included.

*6. Line 105. There is also the "continuation" paradigm, which is probably more popular than the synchronous paradigm with movement scientists including Wing whose model is based on this.*

The synchronization-continuation paradigm, indeed very popular, is now also mentioned.

*7. Line 105-107. It might be better to describe all of these tasks rather than just the one. Again, they refer to somewhat different abilities.*

A description of the different tasks is beyond the scope of this Introduction. The Introduction has now been restructured so that we first mention the different sensorimotor and rhythm perception tasks, adding a few information (no thorough descriptions) so that the reader can have an idea of the specific tasks being used in the literature. We acknowledge that different tasks are likely to refer to different abilities, and the need to have a variety of tests to obtain a thorough timing profile. The anisochrony detection task is presented in detail later as part of the method described in the manuscript.

*8. Line 108+. Is the anisochrony detection task run as a psychophysical forced choice staircase method? If not, then is it correct that one would not know the absolute error between isochronous tones that is detectable by a particular individual? The discrimination index gives a relative ability between the subjects I think. Is that correct? If my thinking is correct, then would it be more useful to adapt the detection task to the staircase method? It would be an easy fix presumably. In this way, you could control for the fact that some individuals might not be capable of "perceiving" a particular asynchronous phasing relationship between beats in the synchronous paradigm.*

The anisochrony detection task as described here is not using a forced-choice staircase method. Thus, the measure of a psychophysical threshold cannot be obtained with this method, also considering the fact that only 3 levels of change (8, 12, and 16% of the IOIs were chosen here), which hardly allow to draw a full psychophysical function. Note, however, that the 3 levels of changes, based on previous literature with isochronous sequences, correspond to asynchronous phasing which is definitely perceived (16%), close or below threshold (8%), and in between (12%). We agree with the Reviewer that using an adaptive procedure to compute the anisochrony detection threshold (with the added value of reducing the duration of the task) would be fruitful. This is indeed what we already did by implementing threshold estimation via a maximum likelihood procedure in a recent battery of tests we developed (the BAASTA), and which includes anisochrony detections tasks. The fact that the anisochrony detection task can be implemented using an adaptive procedure, with reference to our own battery, is now mentioned in the discussion.

*9. Line 280. Exactly how the 10 poor synchronizers were selected is not clear. For example, was this only on consistency or was it accuracy too? It might be better to state inconsistent synchronizers rather than poor synchronizers since the latter terminology implies that they could not exactly time their finger to the beat, which is incorrect according to figure 2. In addition to the negative asynchrony effect, we know from previous work by Thaut and Repp that non-disabled individuals will normally vary more (between themselves) with the accuracy of their timing to the actual beat (phasing relationship to beat) than the accuracy of their ability to keep to the frequency/period of the beat (what is called here synchronization consistency).*

Potential poor synchronizers were selected based on consistency and accuracy. They were individuals with accuracy departing by more than 2 SD from the mean of the screened group, or showing consistency lower than 2 SD from the mean of the group. Thus the term poor synchronization still seems appropriate. This now has been clarified in the text.

After thorough analysis of the synchronization performance of those poor synchronizers as compared to controls, we realized that accuracy did not make a difference between the two groups, whereas consistency did. As mentioned by the Reviewer, indeed, poor synchronizers varied a lot in terms of accuracy; however, this is also true for controls and for this reason group differences were not significant. Hence we decided to use consistency as a measure which non-controversially, and significantly, allowed showin a difference between poor synchronizers and controls, in order to examine individual differences (see Table 1). This has now been clarified in the text.

Finally, we think that “poor synchronizers” is still a good term, and quite appealing for a multidisciplinary readerships, thus we prefer to keep the original terminology. In addition, note that low synchronization consistency is a good indicator of poor synchronization (relatively independent from accuracy). For example, a participant producing taps at times much earlier than the pacing stimuli and at other times significantly lagging after the stimulus in the same tapping sequence would have accuracy (relative phase) around 0. This may lead to the conclusion that s/he is a good synchronizer, which is obviously not the case. However, low consistency (high variability) in this case would point toward poor synchronization.

*10. On a general note, it is not clear to whom this article is directed? Is it clinicians, physical/occupational therapists, music therapists, movement disorder scientists? The audience might influence how terms are used.*

To our knowledge, the readership in JoVE is typically multidisciplinary, thus we preferred general terms, providing definitions when necessary to reach the non-expert in the field.

*11. Lines 286-301. This paragraph would be more compelling with some specific discussion of where the groups do and do not differ statistically in the data presented. At the moment, there are only generalities.*

As mentioned above, statistical tests are not included in the “Representative results” section of the JoVE manuscript accompanying the visual article, but reported in detail in Sowinski & Dalla Bella (2013). Nevertheless, it is now clearly indicated which results are statistically significant in the text.

*12. Lines 303-313. It seems as though only the synchronization task is needed to uncover those who show inconsistent synchronization so the first sentence is misleading. Also it is the inconsistency that is common to all so that is all that is needed. However, it is true that some individuals seem far from the beat in addition. These may be the ones who are really "beat deaf". The others are just variable in the their motor response to a perceptual cue, a key characteristic of clumsy children by the way (children with developmental coordination disorder).*

We agree with the Reviewer that the first sentence is misleading. The section has been totally rewritten. Please see above for the relevance of considering synchronization consistency instead of accuracy.

*13. Lines 315-322. The idea of discovering profiles of timing disorders is appealing but this paragraph does not really give the reader any idea of how this might be useful.*

The paragraph has been developed and extended, and it is made clearer why uncovering profiles of timing disorders is relevant.

*14. Lines 383-389. I am not sure that this is a true advantage for two reasons. First I suspect that people were measuring different timing abilities and secondly you cannot separate action from perception anyway.*

We are afraid, but we disagree with the Reviewer regarding the separability of perception and action. This is obviously possible in terms of the tasks. For example, whereas we agree that a paced tapping task engages both perception and action, an anisochrony detection task (or any other perceptual task) does not require movement to be performed. Secondly, there is a growing amount of data showing separability of perception and action in the pitch domain (in tone deafness/congenital amusia, as we mentioned in the Introduction), and more recently in the timing domain. This point is now made clearer in the Discussion. Yet, we concur with the Reviewer, that in order to have robust and compelling confirmation of the dissociation between perception and action in the timing domain, it would be preferable to have a variety of timing tasks tapping different sensorimotor and perceptual timing abilities. This is exactly the purpose of recent battery of tests (e.g., the BAASTA and the H-BAT). This important point has now been mentioned.

*15. Lines 395-404. I agree that circular statistics are important for the reasons given but the way it is presented sounds like this is a new analysis never before applied to these kind of rhythmic perceptual-motor tasks. It might be better to merely emphasize that this is an easy way to incorporate this valuable statistical analysis.*

We agree with Reviewer, and the text has been changed accordingly.