Reviewer’s report

Title: Best practice for motor imagery: A systematic literature review on motor imagery training elements in five different disciplines

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Reviewer: Aymeric Guillot

Reviewer’s report:

This is a very welcome and interesting paper focusing on the content of a successful motor imagery intervention. While a great amount of research was conducted to investigate the beneficial effects of imagery, it remains difficult to draw final conclusions with regard to the key-components that need to be controlled to ensure the effectiveness of the imagery intervention. For this reason, the present paper deserves attention. Furthermore, this is a well-conducted systematic review, and the authors identified the major characteristics of the imagery intervention. Finally, they found a way to compare the studies whereas there are great differences in the experimental results or the expression of the data between them. In particular, Table 2 is very useful and welcome.

This paper has the potential to become a very important and cited paper in the imagery literature, and further research will certainly take into consideration the present data when implementing a motor imagery intervention. Despite this, however, I may have some major comments for the authors, which might still contribute to improve the quality of the study.

Major Compulsory Revisions

1. While the elements describing the motor imagery training sessions are clearly defined, and comparisons are sound, I would appreciate that the authors go beyond the ‘simple’ comparison between each sub-category. I think that they made the first step by providing evidence for some differences and/or similarities between studies, but I would like reading more conclusions, when possible, to orientate the reader for future studies. For instance, when considering gender differences, is it possible to draw final conclusions with regard to the existing data, and/or to provide more detailed guidelines about how performing well with motor imagery in future studies? This is only an example, and the same reasoning may be further detailed in all sections of the manuscript. Actually, the authors tried to do it in some occasions, but I definitely think it would improve the usefulness of the paper, and further fit with the expectations of readers for such a systematic review.

2. On P. 7, authors state that “study quality was rated between 4 to 9 on a 10-point scale for RCTs, 3 and 6 on an 8-point scale for CCTs, between 4 to 11 on an 11-point scale for CSs, and between 7 to 10 on an 11-point scale for SCRDs”. I am not sure they explicitly explained how they rated the reviewed
studies. This is an important issue, as it may help to objectivate and formalize the ‘degree of importance of detailed motor imagery instructions’ they think readers might have when reading these papers.

3. The authors logically compared the amount of imagery interventions with positive results with that of imagery interventions with no change or negative results. As nicely outlined by Dijkerman et al. (2010)*, published results are almost entirely positive and one does wonder whether there may also have been unpublished studies in which null findings have been obtained. This would be unfortunate, as a comparison between effective and ineffective studies is somewhat biased about which aspects of the imagery training are effective and which group of patients may benefit most. I do think that such statement is absolutely necessary to temper the conclusions one can reach with regard to the direct comparison between effective and ineffective imagery interventions.


4. When comparing the effectiveness of imagery interventions, the authors never mentioned the complexity and/or duration of the task between experimental designs. One may postulate that, for example, few time spent on imagery is enough for simple movements, while more time is needed for more complex ones. I agree that evaluating task complexity is quite difficult and subjective when reviewing imagery studies. However, extra information might be added to temper conclusions regarding the elements for effective imagery training, and explain to the readers that these conclusions might be influenced by the characteristics of the imagined movement.

5. On p. 13, the authors compared whether imagery might be performed after or before physical practice. While I think this aspect is critical, I think authors should take into consideration that it may directly depend on the imagery outcome that the participants are likely to achieve. While in some cases motor imagery should be performed before physical practice (albeit after few actual trials in order to avoid imagining something that has never been performed beforehand), e.g., to learn and memorize surgical procedures, imagery might also be used after (or during) physical practice in other occasions (e.g., when mentally rehearsing a given action in sport in order to achieve peak performance). These are only two examples, but there are many other similar cases that could be imagined. Perhaps it is not possible to conclude on this specific aspect, but rather state that differences may be due to the imagery outcome.

6. I would appreciate that authors include statistics and p-values in figures 11 to 15. Furthermore, how can they explain, for example, that more time was spent on motor imagery in studies with no changes/beneficial effects in sport? Once again, I think the authors should try going a step further, i.e., beyond the ‘simple’ comparison by formulating some hypotheses to explain similarities and differences.
7. Finally, I do think that an important aspect is missing in the review. It is related to motor imagery ability, and the aim is twofold: firstly, there are some inconsistencies in the experimental designs, as in some of them individual imagery ability is adequately evaluated, while in others this is not the case. Secondly, individual motor imagery ability is a possible influencing factor that may impact the successfulness of the imagery intervention. This is therefore to me a very important aspect that should be considered in a systematic review.

Minor essential revisions

1. p. 7, change ‘from an internal view with a kinaesthetic MI mode’ into ‘from an internal view COMBINED with a kinaesthetic MI mode’. There are some inconsistencies in the use of imagery perspectives/types/modalities, and several studies notably confounded internal visual imagery (first-perspective) and kinaesthetic imagery. Caution must therefore be exercised when considering different approaches mixing imagery perspectives (internal and external visual imagery) and imagery types (visual vs. kinaesthetic modes).


Discretionary Revisions

1. I am not totally convinced by the gender effects resulting from the review, and do think that there are not enough data allowing for some definitive conclusions.

Dr. Aymeric Guillot

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests