

Dear Editor,

Thank you very much for sending us the valuable comments of the reviewer and editorial office on our manuscript (Manuscript NO.: 71829: "The effects of hypnotherapy vs. progressive muscle relaxation on reducing test anxiety and attentional bias in medical college students: A randomized controlled trial"), which have helped us to improve the quality of our paper greatly. We have thoroughly revised our manuscript according to the reviewer and editorial office' comments and suggestions. The comments and the critiques of the reviewer and editorial office have been addressed and itemized as follows:

Responses to Reviewer 1

Comments to the Author

This is an interesting and relevant trial studying the effects of hypnotherapy vs. PMR on test anxiety and one of its specific component, attentional bias. The authors hypothesized that hypnosis would fair better over PMR in both regards due to its cognitive components that PMR lacks. This proved to be true in this study. The authors also appropriately commented on state and trait anxiety. The study was well designed and results were clearly presented. The manuscript is very well written, except the abstract section needs some improvement as indicated in comments attached in the manuscript. Discussion section is well thought out and presented. However, one of the conclusions ' Attentional bias may be considered an essential target in treatment of test anxiety or other anxiety disorders, cannot be drawn from this study. The study only

shows that hypnotherapy is superior to PMR in addressing attentional bias. However, the overall improvement in posttest state anxiety between the 2 groups could also be due to other factors that cause test anxiety, and not solely due to improvement in attentional bias. Limitations are duly noted, but it should be mentioned more specifically that lack of physiological measures also makes it difficult to differentiate whether hypnotherapy did better than PMR due to better physical relaxation or attentional bias, or maybe some other factor.

Answer:

We greatly appreciate your valuable comments. We revised the relevant sentences as follows:

“Attentional bias plays an important role in treatment of test anxiety or other anxiety disorders.”

“First, we did not examine participants’ physiological indices, such as skin conductance response, blood pressure, and heart rate. This would provide an objective measure more sensitive to the changes induced by intervention. Moreover, lack of physiological measures also makes it difficult to differentiate whether hypnosis did better than PMR due to better physical relaxation or attentional bias, or maybe some other factor.”

Responses to Reviewer 2

Comments to the Author

This manuscript has a potential impact on the World Journal of psychiatry to introduce the hypnotherapy as a treatment modality of anxiety, this is an interesting paper that presents the benefit of hypnotherapy over the muscle relaxation in aspects of reducing anxiety among medical students. The work appears to have been well done, and

statistical analysis has resulted in interesting outcomes. However, there are a few clarifications that would help with the understanding of their findings.

Answer:

We thank the reviewer for the kind comment. Please see our point-by-point answers below.

1. There are lacking information on the material and method section to describe the demography of participants. The authors did not explain briefly about exclusion and inclusion criteria for participants selection in this study. This is not clear, is there any other factors that may have an impact on TAS score, such as history of taking any medication, chronic illness, and age.

Answer:

We greatly appreciate your valuable suggestion. We revised the relevant sentences as follows:

“Inclusion criteria: participants with a score higher than 20 formed a high anxiety group (n = 102), while participants with a score lower than 12 formed a low anxiety group (n = 62).”

“Exclusion criteria: the therapist (the first author) conducted a semi-structured interview to ensure that all participants did not have a history of psychiatric or neurological disease or history of taking any medication, chronic illness or a current major psychiatric disorder.”

2. References should have to be selective and stringent; author should have to be careful of using the citation of this manuscript. It is difficult to track the cited work, for instance,

in the introduction section, author discuss about attention theory but the claim is not properly cited. A short presentation of visual memory associated with attentional bias approach may be of help, as well as some instances on how attention deficits nowadays envisaged and tackled in current research. Author may cite (DOI: <http://doi.org/10.5334/joc.58>, <https://doi.org/10.1016/j.alcohol.2021.12.001>)

Answer:

We appreciate your kind comment. One reference you suggested is cited, but another reference is not cited because it has little correlation with attentional bias that we focused on. We have revised the relevant sentence as follows:

According to the attention theory that visual memory is closely related to attentional bias^[23], and attentional bias may reflect facilitated orienting of attention to negative information or slowed attentional disengagement from negative information^[24].

3. Author used “/” couple of time instead of “or, and” in introduction.

Answer:

We appreciate your kind comment. We have revised the relevant sentence as follows:

“emotionality or physiology and worry or cognition”.

4. Discussion section is incomplete, authors should need to defend each part of their results like why there is no difference in pretest but they have got a significant difference in their posttest results between treatment and control.

Answer:

We appreciate your kind comment. In the discussion section, we generally do not describe all the results repeatedly, but selectively conduct in-depth analysis and discussion on some key results to get meaningful views. The citation of our own results in the discussion section is similar to the citation of literature. Our own results and the results of others in the literature can be regarded as arguments that we can use in the process of reasoning. Therefore, we have discussed main results like why there is no difference in pretest but they have got a significant difference in their posttest results between treatment and control.

Responses to Reviewer 3

Comments to the Author

World Journal of Psychiatry Manuscript Number: 71829 Title: The effects of hypnotherapy vs. progressive muscle relaxation on reducing test anxiety and attentional bias in medical college students: A randomized controlled trial. The manuscript describes results from a comparative evaluation of hypnotherapy and progressive muscle relaxation to control of anxiety and attention level during tests in a group of medical college students. It is a well-structured study, and the adopted method is very well described, being of great importance the fact that the authors did not forget to mention that all the participants underwent psychiatric evaluation, and a semi-structured interview was used to ensure that any participant did not have a history of psychiatric or neurological disease or a current major psychiatric disorder. This is critical in the case of treatment using hypnosis. I consider this article suitable for publication in WJP. However, I'd like to draw the authors' attention to the fact that the term "hypnotherapy" for the behavioral treatment performed is not appropriate, as hypnotherapy is psychotherapy under hypnosis, being a treatment that uses memory recall in search of the origin of the problem under treatment, and then re-signify it. In

the present research, effectively the technique used was hypnosis with positive suggestions, to enable the participant to perform tests, but the origin of anxiety in each participant was not addressed. The treatment with hypnosis performed was of deconditioning and reconditioning for a specific situation, not psychotherapy under hypnosis. Authors could revise the terminology to improve the text.

Answer:

We greatly appreciate your valuable suggestion. We have revised the terminology (The “hypnotherapy” is replaced by “hypnosis” in most cases) to improve the text.

Responses to Reviewer 4

Comments to the Author

This manuscript presents data from a randomized clinical trial exploring the effect of hypnotherapy and progressive muscle relaxation on test anxiety and attentional bias in medical students. In my view the design seems to be appropriate given the goal of the study. The results confirm efficacy of both methods. However, the paper may benefit from more clear distinction between the compared groups, specifically the active condition, active control and baseline (that might be interpreted as control group per se). This structure might be outlined in the abstract/core tip as well.

Answer:

We appreciate your kind comment. We delete confusing related statements (“as an active control group”, “The PMR group served as an active control group”). The participants were divided into three groups: hypnosis group, PMR group, and control group.

Responses to editorial office’s comments

(1) Science editor:

The manuscript assesses the effects of hypnotherapy and progressive muscle relaxation on test anxiety and attention. This work seems to be well done. The statistical analysis has produced interesting results, which can be considered for publication after revision. It is unacceptable to have more than 3 references from the same journal. To resolve this issue and move forward in the peer-review/publication process, please revise your reference list accordingly.

Language Quality: Grade B (Minor language polishing)

Scientific Quality: Grade B (Very good)

Answer:

We appreciate your kind comment. We delete one reference from the same journal (*Int J Clin Exp Hypn*).

(2) Company editor-in-chief:

I have reviewed the Peer-Review Report, the full text of the manuscript, and the relevant ethics documents, all of which have met the basic publishing requirements of the World Journal of Psychiatry, and the manuscript is conditionally accepted. I have sent the manuscript to the author(s) for its revision according to the Peer-Review Report, Editorial Office's comments and the Criteria for Manuscript Revision by Authors. The title of the manuscript is too long and must be shortened to meet the requirement of the journal (Title: The title should be no more than 18 words). Please provide decomposable Figures (in which all components are movable and editable), organize them into a single

PowerPoint file. Please authors are required to provide standard three-line tables, that is, only the top line, bottom line, and column line are displayed, while other table lines are hidden. The contents of each cell in the table should conform to the editing specifications, and the lines of each row or column of the table should be aligned. Do not use carriage returns or spaces to replace lines or vertical lines and do not segment cell content.

Answer: We appreciate your kind comment.

The title of the manuscript has been shortened to meet the requirement of the journal as following:

“Randomised trial estimating effects of hypnosis vs. progressive muscle relaxation on medical students’ test anxiety and attentional bias”.

We provide decomposable Figures and organized them into a single PowerPoint file.

We modified the table style and provide standard tables.

Yours sincerely,

Xiaoming Li, PhD.