

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	The effects of positive psychology interventions in Arab countries: A protocol for a systematic review
AUTHORS	Basurrah, Asma; Lambert, Louise; Setti, Annalisa; Murphy, Mike; Warren, Meg; Shrestha, Topaz; di Blasi, Zelda

VERSION 1 – REVIEW

REVIEWER	Hartanto, Andree Singapore Management University
REVIEW RETURNED	24-Dec-2020

GENERAL COMMENTS	<p>The authors propose a protocol for a systematic review on the effectiveness of PPIs in the Middle East and North Africa region. A meta-analytic integration is also planned if there is sufficient quantitative data. The planned review is of great importance, and is especially strong in that it extensively covers not just the existing English literature, but also the Arabic literature which is of relevance to the region in question. The research is promising. However, I have a few major concerns in the methodology of the review, detailed below.</p> <p>1. The introduction is generally clear and relevant, with the rationale appropriately explained. The authors may consider improving on the flow within each paragraph so that it sounds less like a list of results; main takeaways should be highlighted instead. Minor grammatical errors should be fixed to improve readability (e.g., on p. 5, “That the positive psychology movement originating in the United States raises concerns ... cultures;” should read “That the positive psychology movement originated in the United states ...”).</p> <p>1. In the methods, the authors seem to consider the Best Possible Self (BPS) intervention as its own discrete PPI instead of a PPI under the optimism category. It is my understanding that BPS is specifically aimed at increasing optimism (see Malouff & Schutte, 2016; Meevissen et al., 2011), and thus the authors should be clearer in their writing in the “Types of interventions” section (top of p. 7). Additionally, it would be good for the authors to clarify if quality of life and life satisfaction are acceptable constructs of well-being in the current review (p. 7). I strongly suggest that the authors include Proquest Dissertations & Theses (and an Arabic equivalent, if available) in their retrieval to ensure that more unpublished research is retrieved.</p> <p>Malouff, J. M., & Schutte, N. S. (2016). Can psychological interventions increase optimism? A meta-analysis. <i>The Journal of</i></p>
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	<p>Positive Psychology, 12(6), 594–604. doi:10.1080/17439760.2016.1221122</p> <p>Meevissen, Y. M. C., Peters, M. L., & Alberts, H. J. E. M. (2011). Become more optimistic by imagining a best possible self: Effects of a two week intervention. <i>Journal of Behavior Therapy and Experimental Psychiatry</i>, 42(3), 371–378. doi:10.1016/j.jbtep.2011.02.012</p> <p>3. Of major concern, there is a discrepancy in the protocol -- on p. 7 the authors state that “We will exclude studies reporting the effects of physical activity interventions”, while on p. 8 the authors state that one of the inclusion criteria is “Included an intervention (training, exercise, therapy) ...” which allows for exercise. The authors should update their protocol accordingly to reflect whether or not exercise/physical activity interventions will be included. The definition of exercise and physical activity intervention should be clarified,</p> <p>4. On p. 9, it is unclear how ‘session duration’ will be recorded. Is it categorical (long vs. short) or continuous (e.g., number of hours/days)? Additionally, will there be any effort made to take into account the frequency and/or total duration of the intervention (e.g., a 5-week daily intervention vs. a 5-week weekly intervention have very different total durations)? More details are required. On p. 10, it is unclear how the authors plan to dichotomise age into ‘children vs. adult’. What is the age cutoff?</p> <p>5. Lastly, the authors did not mention how they will be handling non-independence of data if they conduct a meta-analysis. Non-independence is extremely common and occurs when, for example, the same sample of participants contributes to more than one effect size. For example, a single study could assess optimism via both the Life Orientation Test and another measure of optimism, thereby resulting in two effect sizes contributed by one sample. Multilevel meta-analytic modelling is preferred to address this issue.</p>
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REVIEWER	Motrico, Emma Universidad Loyola Andalucia, Psychology
REVIEW RETURNED	01-Jan-2021

GENERAL COMMENTS	<p>Thank you for inviting me to review the paper " The effects of positive psychology interventions in the Middle East and North Africa region: a protocol for a systematic review". This paper aimed to address an important clinical issue – the interventions based on positive psychology. I have read carefully and found that this study is very carefully created and developed. Although this study has scientific interest, some important aspects should be reviewed by the authors. I hope that my opinions will help shape your research article more precise and interesting. The followings are my comments:</p> <p>1) Abstract: Please, follow PRISMA in the aim and include the “outcome”: “to examine the overall effects of PPIs for both health and clinical populations in the MENA region”.</p> <p>2) Abstract: Please, cite PRISMA in the abstract.</p> <p>3) Abstract: You said: “meta-analysis will be included if outcomes allow; in this instance, subgroups analysis will be included...” Are</p>
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	<p>you doing a “meta-analysis”? If so, more information must be provided.</p> <p>4) Introduction: The theoretical background is clear.</p> <p>5) Aim: Provide an explicit statement of questions being addressed with reference to participants, interventions, comparisons, outcomes, and study design (PICOS).</p> <p>6) Review questions: I do not think that some of the review questions (e.g. variables may influence the effects of PPIs) could be answer by a systematic review. I suggest doing a meta-analysis</p> <p>7) Methods: I do not understand: “We will include empirical studies that used experimental (Randomised Controlled Trials [RCTs], quasi-RCTs, crossover trials, and controlled before-and-after studies) or quasi-experimental designs”</p> <p>8) Methods: Lines 28-31 are related to “Type of outcome”</p> <p>9) Methods: What does “healthy and clinical participants” mean?</p> <p>10) Methods/type of interventions: You said “intervention (training, exercise, therapy) aimed at enhancing positive feelings, positive behaviours, or positive cognitions” I do not understand what you mean. Are you focusing on psychological positive interventions?</p> <p>11) Methods/type of outcome measures: You said “well-being (e.g. happiness, resilience, stress, anxiety or depression)”. Depression, anxiety or depression are not example of well-being.</p> <p>12) The selection process is not clear.</p> <p>13) Subgroup analysis: I do not understand that.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer(s) comments:

Reviewer: 1

- Point 1: The introduction is generally clear and relevant, with the rationale appropriately explained. Response 1. We would like to extend our sincere thanks to you for taking the time to read and review our manuscript. We are very grateful to you for providing your expertise and insight regarding our research study and are glad that you find our introduction to be clear and relevant.
- Point 2: The authors may consider improving on the flow within each paragraph so that it sounds less like a list of results; main takeaways should be highlighted instead. Minor grammatical errors should be fixed to improve readability (e.g., on p. 5, “That the positive psychology movement originating in the United States raises concerns ... cultures;” should read “That the positive psychology movement originated in the United states ...“)]

Response 2. Thank you for pointing out where we may be able to improve the flow. We removed the grammatical errors and have changed the introduction by removing specific effect sizes to improve the flow of the paragraph.

- Point 3: In the methods, the authors seem to consider the Best Possible Self (BPS) intervention as its own discrete PPI instead of a PPI under the optimism category. It is my understanding that BPS is specifically aimed at increasing optimism (see Malouff & Schutte, 2016; Meevissen et al., 2011), and thus the authors should be clearer in their writing in the “Types of interventions” section (top of p. 7)] Response 3. Thank you for clarifying this. We included Best Possible Self (BPS) intervention under the optimism category (Last line on page 6)
- Point 4: it would be good for the authors to clarify if quality of life and life satisfaction are acceptable constructs of well-being in the current review (p. 7)]

Response 4. Thank you for this comment. We have clarified that and included life satisfaction and quality of life under the measure of well-being (page 7, "type of outcome measures" section).

- Point 5: I strongly suggest that the authors include Proquest Dissertations & Theses (and an Arabic equivalent, if available) in their retrieval to ensure that more unpublished research is retrieved]

Response 5: Thank you for this recommendation, we agree that including gray literature will reduce publication bias and increase the validity, reliability and specificity of our findings. We have now included Proquest Dissertation and Theses as part of our database searches (page 7, "Electronic searches" section).

- Point 6: Of major concern, there is a discrepancy in the protocol -- on p. 7 the authors state that "We will exclude studies reporting the effects of physical activity interventions", while on p. 8 the authors state that one of the inclusion criteria is "Included an intervention (training, exercise, therapy) ..." which allows for exercise. The authors should update their protocol accordingly to reflect whether or not exercise/physical activity interventions will be included. The definition of exercise and physical activity intervention should be clarified]

Response 6. We agree with the reviewer that the word "exercise" causes confusion, however, the word "exercise" mentioned in the inclusion criteria refers to positive psychology exercises (activities or interventions) and not physical exercises. To not cause any misunderstanding to the reader, the word "exercise" has been deleted throughout the manuscript.

- Point 7. On p. 9, it is unclear how 'session duration' will be recorded. Is it categorical (long vs. short) or continuous (e.g., number of hours/days)? Additionally, will there be any effort made to take into account the frequency and/or total duration of the intervention (e.g., a 5-week daily intervention vs. a 5-week weekly intervention have very different total durations)? More details are required.

Response 7. Thank you for raising this important point. The "session duration" in the review will be recorded continuously by including number of sessions and duration of session period. We have added this information to the "data abstraction process" section (page 8).

- Point 8. On p. 10, it is unclear how the authors plan to dichotomise age into 'children vs. adult'. What is the age cutoff?

Response 8: We are planning to dichotomise age to Child/ adolescent (up to 17 years old) vs. adult (18 years old and up). We have highlighted this in the "subgroup analysis" section (page 9)

- Point 9. The authors did not mention how they will be handling non-independence of data if they conduct a meta-analysis. Non-independence is extremely common and occurs when, for example, the same sample of participants contributes to more than one effect size. For example, a single study could assess optimism via both the Life Orientation Test and another measure of optimism, thereby resulting in two effect sizes contributed by one sample. Multilevel meta-analytic modelling is preferred to address this issue.

Response 9: Thank you for this point regarding synthesising data where the same sample contributes to more than one effect size. We have added the following paragraph in the Data Synthesis section: 'Multilevel modelling will be conducted to synthesize multiple effect from single studies' (page 9).

Reviewer: 2

We would like to extend our sincere thanks to you for taking the time to read and review our manuscript. We are very grateful to you for providing your expertise and insights regarding our research study.

- Point 1. [Abstract: Please, follow PRISMA in the aim and include the "outcome": "to examine the overall effects of PPIs for both health and clinical populations in the MENA region"]

Response 1: Thank you for raising this point. To follow PRISMA guidelines, we have included the outcomes to the abstract (page 2, "method and analysis" section).

- Point 2. Abstract: Please, cite PRISMA in the abstract]

Response 2. We have cited PRISMA in the Abstract.

- Point 3. Abstract: You said: “meta-analysis will be included if outcomes allow; in this instance, subgroups analysis will be included...” Are you doing a “meta-analysis”? If so, more information must be provided]

Response 3. Thank you for helping us clarify this. We have included the following paragraph in the Data synthesis section: ‘Where possible, quantitative data will be pooled for a meta-analysis and multilevel modelling will be conducted to synthesize multiple effects from single studies’ (page 9).

- Point 4. Aim: Provide an explicit statement of questions being addressed with reference to participants, interventions, comparisons, outcomes, and study design (PICOS)]

Response 4. We agree with the reviewer and have reworded the statement of the review objectives based on (PICO) (page 5).

- Point 5. Review questions: I do not think that some of the review questions (e.g. variables may influence the effects of PPIs) could be answer by a systematic review. I suggest doing a meta-analysis]

Response 5. Thank you, yes we plan to conduct a meta-analysis to examine the effects of PPIs on similar outcomes.

- Point 6. Methods: I do not understand: “We will include empirical studies that used experimental (Randomised Controlled Trials [RCTs], quasi-RCTs, crossover trials, and controlled before-and-after studies) or quasi-experimental designs”

Response 6. We have changed this to make our methodological study inclusion clearer. “We will include empirical studies where individual participants (not groups) have been randomised, there is a control condition, and provided an effect size or enough information to allow us to calculate an effect size” (page 6)

- Point 7. Methods: Lines 28-31 are related to “Type of outcome”

Response 7. Thank you for this comment. We have moved indicated lines to the “type of outcomes” section on page 7.

- Point 8. Methods: What does “healthy and clinical participants” mean?

Response 8. We will include studies where researchers described participants either as healthy, non-clinical or with a clinical condition. In other words, as we are interested in psychological outcomes, rather than specific conditions, we will include all participants.

- Point 9. [Methods/type of interventions: You said “intervention (training, exercise, therapy) aimed at enhancing positive feelings, positive behaviours, or positive cognitions” I do not understand what you mean. Are you focusing on psychological positive interventions?]

Response 9. Yes, we will focus on PPIs that based on positive psychology theory and background. The word "exercise" mentioned refers to positive psychology exercises (activities or interventions) and not physical exercises. To avoid confusion, the word "exercise" has now been deleted.

- Point 10. Methods/type of outcome measures: You said “well-being (e.g. happiness, resilience, stress, anxiety or depression)”. Depression, anxiety or depression are not example of well-being.

Response 10. Thank you for raising this point. We have changed the wording of the relevant sentence to read “The outcomes of interest are well-being, quality of life, resilience, depression, anxiety, and stress” (page 7).

- Point 11. The selection process is not clear

Response 11: Thanks for raising this point. We have reformulated the entire section to make it clearer as indicated in the “Data management and selection process” section on page 8.

- Point 12. Subgroup analysis: I do not understand that

Response 12: We will conduct subgroup analyses to examine moderating effects of four moderators: age group: Child/ adolescent or adult; study population: clinical or non-clinical; type of intervention: single-component or multi-component; duration of intervention: short (<8 weeks) or long (>8 weeks). We clarified this in “Subgroup analyses” section on page 9.

VERSION 2 – REVIEW

REVIEWER	Hartanto, Andree Singapore Management University
REVIEW RETURNED	27-Feb-2021

GENERAL COMMENTS	<p>Overall, I am satisfied with how the authors have addressed my previous comments. I have only a few more concerns for the updated manuscript:</p> <ol style="list-style-type: none">1. The flow of the introduction can be improved upon further; specifically, the third and fourth paragraphs currently still sound like a list of results. The findings of previous reviews are important, but should be synthesised appropriately. The removal of statistics in the third paragraph does little to help the reader understand the previous literature. What is the main takeaway of the previous literature? Perhaps the authors may find it beneficial to think about the purpose of these paragraphs so that they can better convey their message to the reader.2. The updated manuscript needs to be proofread for grammatical errors (e.g., on p. 6, it should be “calculate an effect size”, not the current “calculate and effect size”).3. The authors mention on p. 6 that non-experimental studies will not be included, which is fine. However, they consider “cross-sectional” studies to be non-experimental. Cross-sectional studies can be experimental so it is unclear what the authors mean here.4. In the event that a particular study involves both MENA and non-MENA participants, how will the authors handle this study?
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VERSION 2 – AUTHOR RESPONSE

Reviewer (1)

Dr. Andree Hartanto comments:

Comment: Overall, I am satisfied with how the authors have addressed my previous comments. I have only a few more concerns for the updated manuscript.

Response. We would like to extend our sincere thanks to you for taking the time to read and review our manuscript. We are very glad that you are satisfied with the changes to your previous comments which we found very helpful.

- Point 1: The flow of the introduction can be improved upon further; specifically, the third and fourth paragraphs currently still sound like a list of results. The findings of previous reviews are important, but should be synthesised appropriately. The removal of statistics in the third paragraph does little to help the reader understand the previous literature. What is the main takeaway of the previous literature? Perhaps the authors may find it beneficial to think about the purpose of these paragraphs so that they can better convey their message to the reader.

Response 1. Thank you for pointing out where we can improve the flow. We have made efforts to rewrite the introduction in a way that would convince the reader more.

- Point 2: The updated manuscript needs to be proofread for grammatical errors (e.g., on p. 6, it should be “calculate an effect size”, not the current “calculate and effect size”).

Response 2. Thank you for this comment. We checked the entire manuscript and edited any grammatical errors.

- Point 3: The authors mention on p. 6 that non-experimental studies will not be included, which is fine. However, they consider “cross-sectional” studies to be non-experimental. Cross-sectional studies can be experimental so it is unclear what the authors mean here.

Response 3. Thank you for raising this important point. We have removed cross-sectional studies from the exclusion criteria.

- Point 4: In the event that a particular study involves both MENA and non-MENA participants, how will the authors handle this study?

Response 4: Thank you for raising this important point and helping us clarify this. We have added the following in 'Types of participants': 'When a study includes Arab and non-Arab participants, the study is included if the results of the Arab participants are presented separately'.