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Complementary Therapies in Medicine

journal homepage: www.elsevier.com/locate/ctim

Author response to: Hardly traceable pitfalls in medical review articles

Conducting a systematic review. Should we based our scientific analysis on personal impressions?

The present letter is in response to a letter to the editor entitled "Hardly traceable pitfalls in medical review articles";¹ recently published in *Complementary Therapies in Medicine*. In this letter, co-authors Olaf R. van Loon and A.J. van Loon discussed some allegedly pitfalls in our 2020 systematic review investigating the effects of dry needling in people after stroke,² review that the authors identified as the RVC study.

We appreciate the interest in our work, which represents the first systematic review on the topic. The authors of the letter identified our paper as the best example of a review study following present-day standards despite more recent reviews (even with meta-analysis) have been published in this field.^{3,4} However, they also claimed that several shortcomings could have been avoided with a more thorough review process, as they did for a M.Sc. thesis.

The first and key (supposed) pitfall of the RVC study is what they called possible interdependence of primary publications. This concern is related to three of the 16 studies included in our review, that were identified as the SMS studies.⁵⁻⁷ Olaf R. van Loon and A.J. van Loon stated that some participants included in the SMS studies were the same individuals. Accordingly, we should have reported it. Had we detected that different studies included the same sample population, we would have acknowledged this issue, as we did in other review studies.⁸ However, to our knowledge, current data is not enough to support that statement. As researchers, our work is to carefully gather evidence, discuss their quality, and interpret the clinical impact of the findings to preserve scientific progress. It is not our duty, however, to question the integrity of primary data without evidence to do it so. All three SMS studies had different recruitment periods. Salom-Moreno et al.⁵ recruited participants between January and October 2013, Mendigutia-Gomez et al.⁶ recruited patients from January 2014 to March 2015 and Sánchez-Mila et al.⁷ did it from October 2015 to April 2016. Additionally, the latter of the clinical trials excluded patients that "had received previous treatment with dry needling".⁷ Thus, at

least for this study, it is not possible to assume that participants were the same than in the other two papers. The SMS studies were approved with different Ethics Committee approval's code and had different registries at ClinicalTrials.gov, which again put into question the so-called interdependency of publications. Finally, the trials differed in the clinical characteristics of the sample, e.g., number of participants, mean age, months after stroke, and intervention protocol. Including studies from the same authors (who may be experts in the field) in a review study should not be a bias per se. All in all, the allegedly pitfalls are based on a belief rather than on a certainty. The letter to the editor points out that "our impression is that some of the subjects are included in two or even in three of the SMS studies." Scientific research should not be a matter of

personal impressions. If co-authors of the letter have reasons to believe that there is a scientific misconduct⁹ in any of the SMS studies, they should directly address the authors or the journals where the studies were published and provide data to support their allegations. We believe our review complied with the standards of the A MeaSurement Tool to Assess systematic Reviews-2 (AMSTAR 2) guidelines,¹⁰ as we accounted for the risk of bias and the heterogeneity in individual studies when interpreting the results. This was the main reason not to conduct a meta-analysis, contrary to a recent review in this field that analyzed the same randomized controlled trials.³

The second aspect of the letter (incorrect primary data) relates to the lack of data accuracy in the study by Mendigutia-Gómez et al.⁶ We agree on this concern. Mendigutia-Gomez et al. reported that there were 10 participants in the experimental group, but tables only included data from 9 individuals. We failed to detect this inaccuracy, which has been repeated by the two most recent reviews in the field.^{3,4} Therefore, we commend the authors of the letter for noticing this mistake. Whether this missing participant makes a difference between a statistically significant or not significant result remains unknown but needs to be highlighted.

In conclusion, we understand the concerns about the need to progress with scientific literature when conducting reviews in the health sciences field. This is a shared

responsibility of primary authors, peer reviewers, editors, researchers conducting systematic reviews, and the whole scientific community. Progress in scientific research may be hampered if pitfalls are not properly identified, but also if personal impressions lead our analysis of clinical research.

Declaration of Competing Interest

The authors declare no potential conflicts of interest concerning the research, authorship, and/or publication of this article.

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<https://doi.org/10.1016/j.ctim.2022.102844>

Available online 26 May 2022

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