Clinical Autonomic Research (2019) 29:121–122
https://doi.org/10.1007/s10286-018-0574-3

LETTER TO THE EDITOR

Modulation of cardiac autonomic control by resistance training in human participants

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Received: 12 October 2018 / Accepted: 31 October 2018 / Published online: 10 November 2018

Dear Editors,

We thank Lopes et al. [1] for showing interest in our recent systematic review and meta-analysis [2], which intended to investigate the effect of resistance training (RT) on cardiac autonomic control of healthy and diseased individuals. While we appreciate their meticulous scrutiny of our review, the issues raised by them need to be examined with a different perspective to understand our findings in a more holistic way.

The first point raised by Lopes et al. [1] about the use of uncontrolled studies in our review seems to be a misinterpretation. We did not include any such designs in our review. The review was actually conducted on randomized controlled trials (RCTs), non-randomized controlled trials, and cross-over controlled trials, and the details of those trials are provided in Table 1 of our paper [2]. We admit that studies with different designs were included in the meta-analysis. However, the majority of those studies consisted of randomized and controlled designs (22 of 28 studies); only four were of non-randomized controlled designs, and just two were cross-over controlled trials. We recognize the fact that including studies with different designs might have compromised the findings of the meta-analysis, but only to a minor extent, as the majority of them were RCTs. Nevertheless, we have clearly acknowledged in the paper [2] that the inclusion of designs other than RCTs is a limitation of our review. However, it should also be acknowledged that the use of non-RCTs in such studies is not uncommon. We found that whenever there is a scarcity of RCTs on a concerned topic, even previous systematic reviews and meta-analyses have included studies with different designs [3, 4].

We do not agree with the claim of Lopes et al. [1] that the entire meta-analysis showed high heterogeneity. While this is true for the mean of N–N intervals (mean NN), the ratio of low-frequency to high-frequency power (LF/HF ratio), and the instantaneous beat-to-beat variability (SD1), very low heterogeneity was observed for normalized low-frequency (LFnu) power, high-frequency (HFnu) power, the root mean square of successive differences between adjacent R–R intervals (RMSSD), and the standard deviation of N–N intervals (SDNN) in healthy individuals. Similarly, in diseased individuals, heterogeneity was low for many heart rate variability (HRV) variables such as the mean NN, LF/HF ratio, SD1, and sample entropy. Moreover, despite some heterogeneity, the presence of significant standardized mean differences in almost all comparisons proves that RT had a significant positive impact on the cardiac autonomic control of diseased individuals.

Lopes et al. [1] are right to point out that the inclusion of different diseases in the analysis might have affected the findings. However, our objective—to examine the effect of RT on both healthy and diseased individuals—could only be fulfilled by adopting this approach. All the pathologies addressed in the studies included in the review were characterized by cardiac autonomic dysfunction, as clearly mentioned in the "Methods" section of the review [2]. Moreover, a separate analysis based on the diseases encountered was not possible because an insufficient number of studies were available for each disease.

We would like to reinstate that the number of studies was exactly the same in both the healthy and diseased subgroups

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with regard to SDNN, LF/HF ratio, and SD1, and almost the same with regard to RMSSD (3 versus 4), LF power (5 versus 7), and HF power (5 versus 6). The number of studies was only greatly in favor of healthy individuals when investigating mean NN (5 for healthy versus 2 for diseased subgroups). This, however, did not have any significant impact on any of the subgroup analyses i.e. healthy or diseased. Therefore, it is unlikely that our results could have been affected by the number of studies in each subgroup of the meta-analysis.

We appreciate the earlier systematic review on the topic performed by Kingsley et al. [5], but strongly disagree with the viewpoint of Lopes et al. [1] that our review adds little to the existing literature. The systematic review by Kingsley et al. [5] did not follow the standard Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines and summarized only eight studies on the topic (up to 2013). It also did not include a quality assessment or a meta-analysis. In contrast, our systematic review and meta-analysis was conducted and reported as per the PRISMA guidelines, and it included a rigorous quality assessment and meta-analysis of 28 studies (from inception until 2018). Although our review [2] yielded similar findings to those of Kingsley et al. [5], we would like to emphasize that our review was conducted on a larger body of evidence and with a more comprehensive and rigorous approach to evaluation.

To sum up, the findings of our meta-analysis [2] should be interpreted with some caution, considering the heterogeneity that exists among the studies. In our study, different designs were permitted because of the inclusive nature of our systematic review, and this was the best possible way to present the entire existing literature on the topic. Despite the limitations, our findings clearly showed the positive effect of RT on cardiac autonomic control in the diseased samples. However, large well-controlled clinical trials should be considered in subsequent studies to study the effectiveness of RT on cardiac autonomic control in both healthy and diseased individuals.

Compliance with ethical standards

Conflict of interest The authors declare that they have no competing interests.

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