Efficacy of inspiratory muscle training in chronic heart failure patients ☆

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To the Editor

Smart et al. [1] performed a systematic review and meta-analysis to determine the magnitude of change in peak VO_2, six minute walk distance (6MWD), quality of life, maximal inspiratory pressure (PImax) and ventilatory equivalent for carbon dioxide (V_E/VCO_2 slope) with inspiratory muscle training (IMT) in chronic heart failure (CHF) patients. The authors showed that IMT improves cardio-respiratory fitness and quality of life to a similar magnitude as conventional exercise training and may provide an initial alternative to the more severely de-conditioned CHF patients who may then transition to conventional exercise training. In this letter, the authors point out some of the methodological problems in the study conducted by Smart et al.

First, the PRISMA Statement [2] recommends that the authors present a full electronic search strategy for at least one database, including any limits used. However, this was not demonstrated, which makes it difficult for other researchers to reproduce the study. We [3,4] and Chen and Yin [5] also performed a systematic review and meta-analysis regarding IMT in CHF and 119 and 123 articles were identified for retrieval, respectively, different from the authors who identified only 49 studies. We believe that the search strategy employed by Smart et al. [1] may not have been very sensitive to the research question.

Secondly, the inclusion criteria used by the authors were randomized controlled trials including CHF patients undergoing IMT. However, the studies performed by Laoutaris and coworkers [6–8] were age and sex-matched controlled studies, that is, they were not strictly randomized (what was confirmed through previous contact made by our group with these authors, during the conduction of our systematic reviews [3,4]). Also, although the study performed by Laoutaris et al. [9] was a randomized controlled trial, it included CHF patients after implantation of a ventricular assist device, different from the other studies included in the systematic review. This may introduce selection bias in the systematic review conducted by Smart et al. [1], combining data from patients which are clinically different. Furthermore, the group receiving IMT also underwent moderate-intensity aerobic exercise using a bike or treadmill. Therefore, we believe that the four studies performed by Laoutaris and coworkers [6–9] should not be included in this systematic review and meta-analysis.

Third, in the statistical analysis the authors do not report the effects model used for the analysis—random or fixed (present only in forest plots), and how statistical heterogeneity was assessed [2]. Furthermore, the authors used the Jadad and PEDro scores to assess the methodological quality of included studies. However, the Cochrane Collaboration [10] recommends against the use of scales yielding a summary score. Moreover, the authors do not report if the assessment of methodological quality and data extraction were performed by two reviewers, as recommended [10].

Fourth, there are some problems concerning the results, such as: the authors used a fixed effects model in the meta-analysis, however there is considerable heterogeneity between the included studies and we believe that this model is not the most appropriate one for the analysis. If not used as the primary model, we believe that the authors could at least conduct a sensitivity analysis using the random-effects model. Furthermore, the authors reported that the heterogeneity was low to moderate suggesting that the analyses were appropriate; however, we observed that the analysis of 6MWD and V_E/VCO_2 slope presented high statistical heterogeneity with significant values using Cochran's Q-test and the inconsistency I^2 test, respectively (6MWD: $p = 0.01$, $I^2 = 63\%$; V_E/CO_2 slope: $p = 0.02$, $I^2 = 64\%$). How do the authors explain the high heterogeneity?

Still, we believe that the articles performed by Laoutaris and coworkers [6–9], should not be included in meta-analyses for the reasons explained above. We also think that the study conducted by Winkelmann et al. [11] should not be included in meta-analyses, because it associated another intervention (aerobic exercise) with IMT, therefore discording from the objective of this study, which was to conduct a systematic review of IMT versus sham or sedentary control.

Finally, the authors concluded that IMT improves cardio-respiratory fitness and quality of life as much as conventional exercise training. However, due to the limitations of this study, we believe that these conclusions should be considered cautiously. In the systematic reviews performed by our group, we evaluated the methodological quality descriptively, used a random-effects model for analysis, and did not include the studies conducted by Laoutaris et al. [6–9] and Winkelmann et al. [11] in the meta-analyses, we showed that IMT improves the distance walked in 6MWD (69 m; 95%CI: 7.21 to 130.79) and PImax (23.36 cm H_2O; 95%CI: 11.71 to 35.02), without significant improvement in the peak VO_2 (1.98 ml/kg/min^{-1}; 95%CI: −0.67 to 4.62) in CHF patients [3]. There is also no additional benefit in the quality of life in CHF patients without inspiratory muscle weakness compared to control groups [4].

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