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RESPONSE TO “SPINAL MANIPULATIVE THERAPY AND SPORTS PERFORMANCE ENHANCEMENT: A SYSTEMATIC REVIEW”



To the Editor:

I read with interest the recent paper entitled “Spinal Manipulative Therapy and Sports Performance Enhancement: A Systematic Review.”¹ This study investigated a relevant topic given the competitiveness and physical demand in sporting environments. This systematic review has an important role in identifying main methodological difficulties and directing future research for methodological designs with higher quality.

I have a concern about the methodology in this paper. The results of some studies included in the review suggest that spinal manipulative therapy (SMT) can improve outcomes related to sports performance, but some of these findings are based on within-group differences. In Table 3, it is shown that the handgrip strength of judokas evaluated in the clinical trial developed by Botelho et al² increased up to 16.81% after cervical spine manipulation.

I published a systematic review³ on high-velocity low-amplitude manipulation and athletic performance that analyzed the difference of means and confidence interval (CI) of Botelho et al.² My results showed no differences in handgrip strength values between manipulation and sham groups (the highest difference of means was 2.073 kgf in favor of thrust group [95% CI: -13.58 to 9.68]).

Another point I would like to make is about the inclusion criteria in the systematic review.¹ I feel that there was a lack of specificity in determining the comparison criteria based on the patient, intervention, comparison, outcome strategy that allowed the inclusion of the Costa et al study⁴ comparing the

association of SMT and stretching with only stretching. In this case, the group submitted to SMT and stretching presented an improvement in the outcome related to athletic performance (full-swing); however, this clinical trial does not allow one to identify the isolated effect of SMT.

The authors concluded that 4 of the 7 included studies showed that SMT improved sports performance tests. Among the 4 studies cited are the studies of Botelho et al² and Costa et al,⁴ making me think that the aforementioned limitations may have partially influenced the final conclusion of this review. I agree with the authors in stating in the conclusion that “SMT enhances sports performance is not supported by current evidence” and that “it needs to be better and more deeply investigated.”¹ However, considering the above limitations, I feel it is too subtle to say that “spinal manipulative therapy may be a promising approach for performance enhancement”¹ because the evidence indicates that there is not enough scientific support to use or not to use SMT to improve sports performance.

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RESPONSE TO LETTER TO EDITOR: “SPINAL MANIPULATIVE THERAPY AND SPORTS PERFORMANCE ENHANCEMENT: A SYSTEMATIC REVIEW”



To the Editor:

We are pleased to know about your interest in our paper.¹ We have carefully analyzed your concerns in an attempt to assist in our best capabilities.

The first point raised is a concern that the results found by the Botelho & Andrade paper² are not correct based on another analysis performed by a systematic review.³ However, after analyzing your systematic review,³ we have observed issues that raise fundamental questions regarding its quality and data reliability. These ranged from mixed language terms of Portuguese and English (Table 3),³ to equivocal interpretation of the sham interventions of Botelho & Andrade² and Humphries et al⁴ studies (Table 1).³

Also, it looks like this systematic review³ analyzed the Botelho and Andrade² study results of the "second measure of the left arm" to conclude that there was no difference of grip strength after Spinal Manipulative Therapy (SMT). However we could not find the alleged difference of "2.73 Kg in the second measure of the left arm"^{3(p 418)} within the Botelho and Andrade² study. The difference between first and second measurements of the SMT group was 4.4kgf (left side). If we consider the authors were referring to the second intervention of the SMT group (measures 3 and 4), the difference was 2.2kgf, or if they were comparing SMT and Sham groups after the second intervention the difference was 2.1kgf.

We have analyzed the results described by Botelho and Andrade² and we are in accordance with their conclusions. Grip strength was analyzed in 6 different time points, immediately before and after 3 SMT or Sham interventions. They found that the cervical SMT group had increased grip strength in both hands after the first intervention (baseline and post intervention), and when comparing baseline with the results after the second and third interventions. No differences were found within the Sham group.

The second aspect that you raised is that the inclusion of Costa et al⁵ study "does not allow one to identify the isolated effect of SMT." In our perspective, a systematic review has the purpose to be as inclusive as possible to fulfill its scientific role to condense research about the same theme. Therefore, we felt that it was mandatory to include this study, the only different aspect between the groups was SMT (stretching was being applied to both groups). Many times researches will be testing interventions on the top of training routines, which may include stretching and strengthening

exercises, and several other aspects that will not allow the researcher to evaluate the "isolated effect of the SMT," as it could theoretically be done in a laboratory environment.

Regarding the last sentence of your comments about our statement¹ that "spinal manipulative therapy may be a promising approach for performance enhancement," it is important to note that this was not made merely based on the number of studies showing "positive" results towards SMT, but to the great number of studies showing several neurophysiological effects of SMT that could potentially influence sports performance associated to the few clinical trials in where this theoretical frame was actually observed.

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