Letter to the Editor

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On Cobb angles, flexibility, and lumbar strength measurements

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To the editor:

The authors would like to thank Ko and Kang for their contribution to the literature in their article titled "Effects of 12-week core stabilization exercise on the Cobb angle and lumbar muscle strength of adolescents with idiopathic scoliosis" (Ko and Kang, 2017). However, we believe that the conclusions drawn by these authors are not adequately supported by their study findings.

Research into the effects of exercise as an intervention for adolescent idiopathic scoliosis (AIS) is, without question, an important pursuit given the impact that this condition has on patients, and the direct and indirect costs involved with the management of AIS. Research into this condition serves to inform both practitioners and patients of treatment options which can range from conservative strategies, e.g., exercise, all the way through to highly invasive surgical procedures.

In Ko and Kang's study, radiographic Cobb angles, flexibility and lumbar strength measurements were taken in a group (control and intervention) of primary school students before and after a 12-week core stabilization exercise program. The results from a control group were compared to those in the exercise group. The authors report that there were significant changes in the Lumbar Cobb angles of the exercise group. The figures that have been presented in Table 2 of the manuscript indicate that the preintervention mean and standard deviation lumbar Cobb angle in the exercise group was 15.95°±1.84°, and then 15.21°±1.91° after 12 weeks. Using a repeated-measure two-way analysis of variance the authors reported statistically significant differences (alpha=0.05) in these pre- and post-Cobb angles. The authors then went on to conclude that core stabilization exercise is effective for reducing the Cobb angle in patients with AIS.

Given that the measurement error for radiographic Cobb angle measurement (Langensiepen et al., 2013; Morrissy et al., 1990) and diurnal variation (Beauchamp et al., 1993) may be as high as 5°, and the minimal clinically important difference is considered to be ≥5° (Carman et al., 1990), it is difficult to understand how Ko and Kang can make such strong statements regarding efficacy. Furthermore, the study is not a randomised controlled trial which further limits the strength of the authors' conclusions.

We would recommend that Ko and Kang revise their conclusions to more closely align with the robustness of their research findings and the inherent limitations associated with their chosen research design so as not to confound the evidence in this important area.

CONFLICT OF INTEREST

No potential conflict of interest relevant to this article was reported.

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