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Letter to the Editor: Comparison of the effects of dry needling and spinal manipulative therapy versus spinal manipulative therapy alone on functional disability and endurance in patients with nonspecific chronic low back pain

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Dear Editor,

We read with great interest the study by Khan et al.,[1] which aimed to evaluate the additional effect of dry needling compared to spinal manipulative therapy alone in patients with chronic non-specific low back pain. Considering the use of dry needling in clinical practice and the fact that its effect on functional disability remains controversial,[2] further research into its efficacy remains pertinent. However, we believe that the conclusions drawn by Khan et al. are potentially misleading due to multiple misinterpretations and significant methodological concerns.

Firstly, the study reported on disability and muscle endurance outcomes although the clinical trial registry (https://irct.behdasht.gov.ir/trial/61988) listed also pain intensity and range of motion as primary outcomes; the results of these latter are, however, not reported by the authors in the published article, raising concerns regarding selective reporting bias. Additionally, the description of the spinal manipulative therapy varies between the published article, where it is defined as "high-velocity low-amplitude force" and the clinical trial registry, which describes it as "joint mobilization or manipulation techniques applied to the spine or pelvis, with the particular dose and techniques at the discretion of the treating physical therapist, based on each participant's physical examination findings". Moreover, the term "spinal manipulative therapy" is misleading, as the intervention comprised only a single manipulation, which does not align with the usual spinal manipulative treatment in clinical practice.

Secondly, a closer examination of the results section reveals several inconsistencies. Notably, the flow diagram indicates that 128 participants were assessed for eligibility and that 10 were excluded, yet the total number of randomized participants remains 128.

Furthermore, within both experimental groups (n = 64), 20 participants are recorded as having received the allocated intervention, while none did not receive it, raising concerns regarding data integrity. More problematic are the implausible baseline data for the Sorensen test, with mean endurance times of 2.5297 and 3.1469 seconds in the respective groups, whereas typical Sorensen endurance times for this population generally exceed 60 seconds.[3]

Thirdly, the absence of mean change and effect sizes calculations, which are essential for quantifying the magnitude of the effect,[4] as well as the lack of interpretation regarding the clinical relevance, further limit the study's validity. Without such analyses, it remains unclear whether the observed changes hold meaningful clinical significance.

Finally, the authors concluded that "both therapies effectively reduced low back pain", neglecting the fact that there was no placebo or waiting-list control group. The literature consistently highlights that multiple factors, including natural history, regression to the mean, Hawthorne effect, etc... may contribute to patient improvement independent of the applied intervention.[5]

Despite these substantial concerns, the authors recognize only one limitation: the absence of long-term follow-up.

Therefore, we strongly advocate for a cautious interpretation of the study findings.

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