

Comments on “The Efficiency of Manual Therapy and Sacroiliac and Lumbar Exercises in Patients with Sacroiliac Joint Dysfunction Syndrome”

To the Editor:

We read with interest the study by Javadov et al (1) where different treatment modalities were applied to patients with sacroiliac joint dysfunction syndrome. Compared to the bulk of literature regarding the management of people suffering from low back pain, there is indeed an urgent need to better understand and manage people with pelvic girdle pain. However, we feel that the conclusions of the article of Javadov et al. are misleading due to different important methodological considerations.

Firstly, important information regarding (the lack of) blinding of the assessor, sample size calculation, primary versus secondary outcome measures, reliability of the clinical tests, etc. is missing. This is however crucial to estimate the risk of bias of this study. Secondly, the authors point out the lack of healthy control group as a study limitation, which is surprising seeing as the scope of the paper is to explore different treatment options in patients. It would however have been interesting to compare the results of the different treatment options with a group of patients receiving no or placebo treatment. Thirdly, despite the fact that plenty of evidence demonstrated the lack of clinimetric properties of sacroiliac joint mobility tests (2), and that literature recommends using a cluster of pain provocation tests (3), the authors still used sacroiliac mobility tests and interpreted the tests in isolation. Fourthly, it is absolutely unclear why authors have used outcome measures such as the DN4. A rationale to use this questionnaire is completely missing. In contrast, it would have been relevant to investigate other important outcomes such as the psychosocial factors as it is recommended to consider pelvic girdle pain from a biopsychosocial

perspective (4-5); besides, pain catastrophizing is now included in the pelvic Girdle Pain Core Outcome Set (6). Having included such outcomes would have resulted in a broader view on the patients' baseline characteristics as well as on the changes resulting from the treatment (with a proper sample size calculation and definition of primary outcome). Finally, the authors report as a conclusion of the abstract that manual therapy is effective in the long-term for sacroiliac dysfunction syndrome, which cannot be linked to their design, seeing that the last evaluation was only after 90 days. The absence of effect sizes to reveal the magnitude of the effect is another important limitation of this study (7).

For all these reasons, we believe that the conclusions of this article are misleading.

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