Do these large numbers contrast multiple smaller-number prior studies?

RE: Kauther et al. 2012, Cervical range of motion and strength in 4,293 young male adults with chronic neck pain. *European Spine Journal* 21:1522–1527

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The exceptional number 4293 drew my attention in Kauther et al.'s title [1]. However, as early as the abstract, the number diminished from 4,293 to 746 patients with neck pain, with the rest of the population treated as control subjects. Beyond the misleading title, several other limitations of this work became apparent after reading the whole paper in detail. First, Kauther et al. [1] claimed that their primary goal was to investigate a possible correlation between cervical biomechanics and neck pain. However, no correlation analysis was presented in the results or anywhere else in the text. Furthermore, the authors hypothesized that there is no impairment in range of motion (ROM) and strength in patients with neck pain, yet suggested treatment by neck muscle exercises. Why provide therapeutic exercise if there is no impairment? This hypothesis contradicts multiple previous studies showing impairment in strength and ROM in patients with chronic neck pain [2–4], which was accepted by the American Physical Therapy Association as part of their guidelines [5].

The reported study population included “746 patients with conservatively treated radiating neck pain” [1]. Interestingly, the titled chronic neck pain changed here to patients with radiating neck pain? Is this radiculopathy? Referred pain? Radiating to the arm? Hand? Moreover, the word chronic is absent from the description of the study population. Another descriptive problem is that the authors used adjectives such as ‘healthy’ and ‘normal’ to describe the control group, which should have been avoided,

considering that the control subjects were patients with low back pain (LBP) or other musculoskeletal disorders undergoing physiotherapy training. It seems very likely that patients with spinal problems will present with impairments similar to patients with neck pain. Rather than merely stating at the end of the paper, as the single limitation of the study, that most of the control subjects might have suffered from LBP, the authors should have changed the title and the whole paper to a comparison between neck pain patients and LBP patients. As it stands now, the findings are not comparable with prior literature reviewed in the discussion.

Spine surgery was the only exclusion criterion reported. Lack of defined inclusion and exclusion criteria for the population may bias any findings. As for measurement tools, cervical mobility was measured using a ‘Cervical Measurement System’, for which no information about the manufacturer is provided. In the statistical analysis section, a *t* test was used to compare between groups. However, analysis of variance (ANOVA) would be the appropriate method to compare between two uneven groups.

In the results, the authors reported no significant difference between the neck pain and ‘control’ groups. Interestingly, the two groups have average values that are almost identical. Large groups may bring averages closer together, but such small differences between patients with neck pain and other patients are difficult to explain. Despite the time and effort that this scale of data collection demands, we should not compromise on methodological and ethical standards. As researchers and clinicians, we need to critically appraise our work and aim for high research standards to provide a valid evidence base.

Conflict of interest None.

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