



Letter to the Editor: Lumbar transforaminal injection of steroids versus platelet-rich plasma for prolapse lumbar intervertebral disc with radiculopathy: a randomized double-blind controlled pilot study

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Dear Editor,

We read the article entitled “Lumbar transformation injection of steroids versus platelet-rich plasma for prolapse lumbar intervertebral disc with radiculopathy: a randomized double-blind controlled pilot study” by Gupta et al. [1] with interest. However, we would like to comment on its content and statistical analysis reported.

Firstly, we would like to draw attention to the title which should be written according to PICO (Population/Patient/Problem, Intervention, Comparison, and Outcome) guidelines, it could be reframed as, “To compare the effectiveness of lumbar transforaminal injection of steroids and platelet-rich plasma in patients with prolapsed lumbar intervertebral disc induced radiculopathy: a double-blinded randomized clinical trial” [2].

Secondly, we would like to appraise the information provided in the introduction part regarding the lumbar transforaminal injection of steroids and platelet-rich plasma and its relation to the prolapsed lumbar intervertebral disc. Our understanding is expanded

based on various routes of drug delivery, hazard ratio, and benefits. The study hypothesis has not been acknowledged by the authors. However, we believe that this should be a two-tailed hypothesis: there might be a significant difference in the efficacy of lumbar transforaminal injection of steroids and platelet-rich plasma to improve radiculopathy in patients with prolapsed lumbar intervertebral disc as an alternate hypothesis and the null hypothesis could have been there might not be a significant difference in the efficacy of lumbar transforaminal injection of steroids and platelet-rich plasma to improve radiculopathy in patients with prolapsed lumbar intervertebral disc [3].

However, the stated inclusion and exclusion criteria need to be modified. The specific age bar at which research participants were included and the amount of time since the disease's inception was absent from the inclusion criteria. Nonetheless, we appreciate the authors' reference to the red flags, which clarifies the situation and advances our understanding.

Thirdly, the sampling method, presumably convenience sampling, should be explicitly stated. In the

statistical analysis section, it is important to mention the test used to assess normal distribution, with consideration given to the appropriateness of the Mann-Whitney *U* test for non-normally distributed data [4]. Additionally, the authors should specify the software used for sample size calculation, providing transparency and reproducibility of the study's methods. The data analysis was difficult to comprehend because it was unclear, for which outcome measures parametric or non-parametric tests were used.

However, the discussion and results are consistent with other recent evidence that supports the notion that plasma-rich platelet is a useful alternative to steroids for lumbar transforaminal injection in reducing lumbar radiculopathy in individuals suffering from prolapsed lumbar intervertebral disc.

We are curious about the author's thoughts on these remarks.

Conflict of Interest

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

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All authors provided the same amount of effort for the preparation of this commentary.

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