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LETTER

Liposomal Bupivacaine Infiltration and Postoperative Pain Outcomes in Lumbar Fusion: A Prospective Randomized Controlled Trial [Letter]

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Dear editor

We read with interest the study by Zhang et al comparing liposomal bupivacaine plus plain bupivacaine (LB+B) versus ropivacaine (RH) for wound infiltration in lumbar fusion surgery.[1] The authors concluded that LB+B did not provide a significant analgesic advantage. Although the trial addresses an important clinical question and is generally a well-designed study, several methodological and reporting issues warrant attention, as they may affect the interpretation and generalizability of the findings.

Firstly, an undefined group abbreviation (BH group) appears in the manuscript. In the narrative of the Results section (page 6, right column, end of the second paragraph), an abbreviation "BH group" appears that is neither defined nor used elsewhere in the paper. The original text states: "Although the BH group was 106 μg intravenous morphine equivalents…" Based on logical inference from the context, this is very likely a typographical error for "RH group" (ropivacaine group). Although this error does not affect the direction of the study's conclusions, it may undermine readers' confidence in the accuracy of the study details.

Secondly, there is an inconsistency between the pre-registered primary endpoint and the primary inference drawn from the study.The trial was pre-registered, specifying the 24-hour resting VAS score as the primary endpoint, and the sample size was calculated accordingly. However, the main conclusions are drawn from generalized estimating equation (GEE) models evaluating the overall treatment effect and the treatment-by-time interaction, whereas the pre-specified primary endpoint (24-hour resting VAS) is not directly reported as a between-group comparison in the manuscript. This analytical approach deviates from the pre-specified analysis plan without clear justification and may increase the risk of type I error due to multiplicity.[2] The authors should first report the unadjusted analysis of the pre-specified primary endpoint and then present the GEE results as sensitivity or secondary analyses.

Thirdly, the attrition rate exceeded the planned rate, and missing data were inadequately handled. The authors anticipated a 10% dropout rate, but the modified intention-to-treat (mITT) analysis included only 83 of 94 randomized patients (11.7% attrition). More importantly, the reasons for loss to follow-up were not compared between groups, and no sensitivity analyses were performed. In pain research, where subjective outcomes predominate, missing data are often non-random. Sterne et al emphasized that sensitivity analyses should be used to assess the impact of different missing data mechanisms on conclusions.[3] Without evaluating whether attrition differs between treatment groups or by pain levels, the possibility of selection bias cannot be excluded.

Fourthly, the sample size calculation was based on an extremely small pilot study.The sample size was derived from a pilot study with only five patients per group, which yielded a Cohen's d of 0.87. It is well known that small pilot studies produce inflated and imprecise effect size estimates due to sampling variability. Button et al demonstrated that small samples tend to overestimate true effect sizes, leading to underpowered confirmatory trials and a substantially increased

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risk of type II error.[4] The final trial observed a much smaller effect (eg., β=−0.27 for resting VAS), further suggesting that the study was likely underpowered to detect a small but potentially clinically meaningful difference.

Overall, this study provides valuable negative evidence suggesting that, in the context of multimodal analgesia, LB+B does not offer a clear clinical advantage over ropivacaine. However, given the multiple methodological limitations-including a typographical error, endpoint switching, inadequate handling of attrition, and underpowered sample size-the negative findings should be interpreted as inconclusive rather than as evidence of equivalence. We recommend that the authors correct the typographical error in the manuscript and provide group-specific reasons for loss to follow-up as well as sensitivity analyses for missing data, so that readers can fully assess the robustness of the results.

Generative AI Statement

The author declares that the artificial intelligence tool DeepSeek-V3.2 was used for polishing and translating this communication.

Disclosure

The authors declare no conflicts of interest in this communication.

References

1. Zhang SJ, Lu X, Liu TX, et al. Liposomal bupivacaine infiltration and postoperative pain outcomes in lumbar fusion: a prospective randomized controlled trial. *J Pain Res*. 2026;19:1–12. doi:10.2147/JPR.S598576
2. Schulz KF, Altman DG, Moher D, CONSORT Group. CONSORT 2010 statement: updated guidelines for reporting parallel group randomised trials. *BMC Med*. 2010;8:18. doi:10.1186/1741-7015-8-18
3. Sterne JA, White IR, Carlin JB, et al. Multiple imputation for missing data in epidemiological and clinical research: potential and pitfalls. *BMJ*. 2009;338(b2393):b2393–b2393. doi:10.1136/bmj.b2393
4. Button KS, Ioannidis JP, Mokrysz C, et al. Power failure: why small sample size undermines the reliability of neuroscience. *Nat Rev Neurosci*. 2013;14(5):365–376. doi:10.1038/nrn3475

https://doi.org/10.2147/JPR.S619878