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Response to the Letter to the Editor on "Role of Prophylactic Negative Pressure Wound Therapy in Reducing Surgical-Site Infections in Spine Surgery – A Systematic Review and Meta-Analysis"

Dear Editor,

We appreciate the opportunity to respond to the recent letter addressing our systematic review and meta-analysis on prophylactic negative pressure wound therapy (NPWT) in spine surgery.[1] We thank the correspondents for their careful appraisal and for raising important methodological considerations regarding the inclusion of historically controlled studies, particularly the work by Imtiaz et al[2]

The correspondents rightly highlight that historically controlled designs are susceptible to secular trend bias, institutional practice evolution, and confounding by indication. These limitations are well recognised in the methodological literature, and we acknowledge that the Imtiaz study contributed a directionally discordant effect and modest statistical weight to the pooled estimate of surgical site infection (SSI). While our analysis employed random-effects modelling with Knapp-Hartung adjustment to account for heterogeneity, the inclusion of such studies inevitably raises questions about causal interpretability.

Our rationale for including all eligible comparative studies was grounded in transparency and comprehensiveness. The evidence base for NPWT in spine surgery remains relatively limited, with randomised controlled trials (RCTs) few in number. Excluding historically controlled studies outright would have narrowed the scope of available data and potentially overlooked signals relevant to clinical practice. Nevertheless, we agree that sensitivity analyses excluding such cohorts would provide valuable insight into the robustness of pooled estimates. Indeed, our subgroup analyses already demonstrated consistent benefit across prospective and randomized designs, reinforcing the protective association of NPWT with reduced SSI.

The correspondents correctly infer that exclusion of the Imtiaz study would likely strengthen the observed protective effect and reduce heterogeneity. This aligns with our exploratory analyses, which suggested that the pooled odds ratio shifted further below unity when historically controlled cohorts were removed. However, we felt it important to present the inclusive analysis first, while acknowledging the limitations, so that readers could appreciate both the promise and the current uncertainties surrounding NPWT.

It is also important to recognize that the Imtiaz study, despite its methodological limitations, reflects real-world practice in a large institutional cohort. Such data, while imperfect, can provide context for how NPWT performs outside of controlled trial settings. Balancing methodological rigor with clinical relevance is a perennial challenge in evidence synthesis, and our approach sought to maximize inclusivity while maintaining transparency about study design heterogeneity.

We concur wholeheartedly with the correspondents' recommendation that future meta-analyses prespecify sensitivity analyses excluding historically controlled cohorts. This would strengthen causal interpretability and enhance confidence in translating findings into practice. Moreover, as larger multicentric RCTs become available, reliance on retrospective and historical comparators will diminish, allowing for more definitive conclusions.

Beyond methodological considerations, the correspondents' letter underscores the broader imperative of critical appraisal in evidence synthesis. Meta-analyses are powerful tools, but their conclusions are only as robust as the studies they include.[3] By highlighting the potential influence of a single discordant study, the correspondents remind us of the importance of sensitivity analyses, subgroup exploration, and transparent reporting.[4] These practices not only strengthen the scientific validity of meta-analyses but also enhance their utility for clinicians making real-world decisions.

We also wish to emphasise that our findings should be interpreted in the context of patient selection. NPWT may be most beneficial in high-risk cohorts, such as patients undergoing multilevel fusion, those with diabetes or obesity, or those requiring extensive instrumentation. The correspondents' observations reinforce the need for careful patient stratification in both clinical practice and future research. Larger multicentric RCTs with standardised protocols will be critical to confirm the routine role of NPWT and to refine its cost-effectiveness profile.

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In conclusion, we thank the correspondents for their thoughtful engagement with our work. Their insights enrich the ongoing discourse on methodological rigour and clinical translation in perioperative spine care. We remain committed to advancing the evidence base for NPWT, and we view this exchange as a valuable step toward refining both the science and the practice of infection prevention in spine surgery.

Sincerely,
Dr Sathish Muthu, MS, DNB, PhD
On behalf of all authors

Sathish Muthu, MS, DNB, PhD[1,2], Vibhu Krishnan Viswanathan, MS[1,3] and Dhibin Vikash Kolarpatti Ponnusamy, BS[1,4]

[1]*Department of Spine Surgery, Orthopaedic Research Group, Coimbatore, India*
[2]*Central Research Laboratory, Aarupadai Veedu Medical College and Hospital, Vinayaka Mission's Research Foundation (Deemed to be University), Puducherry, India*
[3]*Department of Orthopaedic Surgery, University of Alabama, Birmingham, AL, USA*
[4]*Department of Orthopaedics, Government Medical College, Karur, India*

ORCID iDs

Sathish Muthu https://orcid.org/0000-0002-7143-4354
Vibhu Krishnan Viswanathan https://orcid.org/0000-0002-3804-1698
Dhibin Vikash Kolarpatti Ponnusamy https://orcid.org/0000-0003-1071-0084

Data Availability Statement

Data generated in the study will be made available upon reasonable request to the authors.

References

1. Muthu S, Viswanathan VK, Munisamy S, et al. Role of prophylactic negative pressure wound therapy in reducing surgical-site infections in spine surgery - a systematic review and meta-analysis. *Glob Spine J*. 2026; 21925682261426264. doi:10.1177/21925682261426264. Published online February 11 2026.
2. Imtiaz H, Ali C, Noordeen H, Anwar H. PICO^{TM} (Closed-Incision negative-pressure wound therapy) dressing use as post-operative prophylaxis for preventing surgical site infections in spinal surgery: a retrospective single-centre Study. *Cureus*. 2024; 16(9):e69214. doi:10.7759/cureus.69214.
3. Sathish M, Eswar R. Systematic reviews and meta-analysis in spine surgery-how good are they in methodological quality? A systematic review. *Glob Spine J*. 2021;11(3):378-399. doi:10.1177/2192568220906810.
4. Aung NM, Jurak I, Mehmood S, Axon E. Sensitivity analysis in meta-analysis: a tutorial. *Cochrane Evid Synth Methods*. 2026; 4(1):e70067. doi:10.1002/cesm.70067.