



Commentary is not a substitute for evidence generation

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We thank Abudayeh and Fishchenko for their interest in our systematic review on the management of coccygodynia (1).

It is always easy to criticise when one has not undertaken the work. The correspondence by Abudayeh and Fishchenko appears to stem from a misunderstanding of the well-defined and express methodological boundaries of our systematic review. As clearly specified in the main body of the article, our work was intentionally restricted to clinical trials: this was expressly stated in the title, eligibility criteria, and protocol. What Abudayeh and Fishchenko describe as an “underrepresentation” of surgical evidence is simply the result of a predefined methodological scope, not an omission. Their critique, therefore, cannot address a flaw, but bears witness to the fact that they misread the explicit design of the study itself.

The suggestion that our review “minimises” the role of surgery is equally unfounded. On the contrary, we explicitly acknowledge that coccygectomy remains an effective option in selected patients with structural pathology and persistent symptoms. However, it is essential to distinguish efficacy from reflexive overuse. Surgery is often the most visible

solution, not necessarily the most intellectually rigorous. In coccygodynia, coccygectomy may indeed resolve symptoms, yet it can also reflect a limited understanding of the multifactorial nature of the condition. The temptation to “remove the problem” rather than to diagnose and correct its biomechanical or functional drivers is well known, and it risks oversimplifying an ailment that requires careful evaluation and well-thought-out, graduated management. A surgical procedure should never substitute for clinical reasoning.

Contrary to the authors’ implication, our review does not diminish the role of surgery; it contextualises it responsibly within a well-defined hierarchy of evidence. The letter by Abudayeh and Fishchenko merely reiterates the literature already cited in our discussion; it offers no new insights, no methodological refinements, and no alternative analysis. Should they believe that a broader synthesis is needed, the appropriate academic response is to produce such work, not to critique a peer-reviewed systematic review for adhering to its own declared methodology.

The scientific conduct of the correspondents, as reflected

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in their publication history, raises broader concerns that extend well beyond the specific context of our article. A simple PubMed search using their surnames and initials clearly shows that their entire indexed output consists of seven and eight publications, respectively, all of which are comments or letters criticising the work of other authors. Not a single prospective study, clinical trial, case series, interventional analysis, review, or even a case report authored by either Abudayeh and Fishchenko is available in the biomedical literature. Their academic presence is therefore entirely built on commenting on the work of others rather than contributing original data. This pattern is not merely unusual; it indicates a structural imbalance between critique and scientific responsibility. Scientific discourse requires participation, not solely passive commentary.

In research-active fields such as orthopaedics and spine surgery, scholarly criticism carries weight only when grounded in a demonstrable understanding of study design, evidence generation, and methodological execution. We respectfully posit that a publication record composed exclusively of letters does not provide such grounding. Commenting repeatedly on the work of others without producing substantive studies implies an asymmetric engagement with scientific discourse: in clinical sciences, it is significantly easier to critique than to conduct a trial, recruit patients, analyze outcomes, or undergo rigorous peer review for a full manuscript. This asymmetry becomes problematic when authors who have not contributed original data attempt to position themselves as authorities on methodological completeness.

More importantly, this pattern raises questions about the motivation behind the correspondence. Publishing letters is, for some, an expedient method to accumulate PubMed entries with minimal effort. A short commentary requires neither data acquisition nor methodological rigour; it undergoes a simplified editorial process, and it allows authors to insert their names into a field without the burden of conducting actual research. When such letters provide substantive critique or highlight relevant methodological blind spots, they can still serve a function. However, when they reiterate information already covered in the primary article and offer no new evidence, they risk becoming an opportunistic means to obtain visibility rather than a legitimate scientific contribution.

This leads to a broader reflection on the role of journals in enabling such practices. A journal bears the responsibility of filtering correspondence to ensure that letters genuinely

advance discourse. Allowing publication of letters that neither present new data nor introduce conceptual or methodological insights weakens academic standards and inflates the appearance of scientific productivity. If a letter merely restates data already cited in the article or expresses dissatisfaction with a methodological decision explicitly stated in the protocol, the editorial process should question its necessity. The purpose of correspondence is to contribute, not to reproduce.

Moreover, journals should be aware of the growing trend in which authors with minimal research output rely almost exclusively on correspondence as their pathway into the academic ecosystem. This phenomenon is particularly evident when the publication pattern consists solely of brief letters across multiple topics, without any scientific depth. Such a trajectory may generate a façade of academic engagement, but it does not reflect true expertise or scientific investment. Editors should therefore carefully discern between letters that legitimately question the methodology or the conclusions of an article and those that use the correspondence section as a vehicle for visibility.

The lack of original contributions from the correspondents becomes even more significant when contrasted with the expectations for evidence generation in the field. Coccygodynia is a condition in which rigorous trials are challenging to conduct, and observational cohorts have historically dominated the literature. If the correspondents genuinely believe that the field requires broader syntheses, deeper methodological exploration, or new interventional paradigms, the most coherent and responsible academic response would be to undertake such work. This would involve designing prospective studies, selecting cohorts, conducting comparative analyses, or developing systematic frameworks capable of addressing the very gaps they highlight. Critique unaccompanied by action contributes little to scientific progress.

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