24-Dec-2022
BMJ-2022-070730.R1
Surgical versus non-surgical treatment for sciatica: a systematic review and meta-analysis of randomised controlled trials

Dear Dr. Liu

Thank you for sending us your revised paper and for your patience while we have been considering it.

We still recognise the potential importance of your paper and its relevance to general medical readers, but we have not yet been able to reach a final decision on it because several important aspects of the work still need clarifying.

We sent it back to the original peer reviewers and discussed it among editors. We believe that the revisions have improved the manuscript and addressed a number of our comments from the first round of review, however a few queries still remain.

We hope very much that you will be willing and able to revise your paper in response to the peer reviewers and editors listed below. Please do not hestitate to get in touch if you wish to discuss any of the comments. We are looking forward to reading the revised version and, we hope, reaching a decision.

Yours sincerely,

Dr Navjoyt Ladher
Research Editor
nladher@bmj.com

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Comments from editors

Thank you for your hard work on the resubmitted manuscript. Your revisions have improved the manuscript, and we are reassured that your paper is adding to earlier work. However we note that there are still some concerns raised by the peer reviewers.

We felt that the two key points raised by reviewers could be discussed in more depth in the manuscript, specifically:

1. The heterogeneity of non-surgical treatment options

2. The framing of the research question and population

If possible, we would suggest revising the paper so that these issues are explored and discussed with more nuance.

** Comments from the external peer reviewers**

Reviewer: 1

Recommendation:

Comments:
The authors have addressed some of my findings. Their answer that a 2020 Cochrane review found little difference between ESI types is not sufficient (I was an author on that paper). If there were no differences, there would be no transforaminal ESI since these are riskier. There are other reviews that find different outcomes, and TFESI are generally not used for bilateral pain (unless someone does 2 injections).

As someone who does intradiscal injections, I also cannot agree with lumping a percutaneous intradiscal injection with open spine surgery. This opens up all sorts of possibilities (what about nucleoplasty or other percutaneous treatments). These procedures do not require an operating room (can be done in a simple fluoroscopy suite).

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Reviewer: 2

Recommendation:

Comments:
I would like to thank the authors for revising their manuscript. I believe it has further improved. I have some additional small comments, but importantly have to insist on my original concern about the selective population reported on in this meta-analysis and the misleading title/conclusions. I do not think the authors have addressed my concern and I do think this needs to be addressed more carefully before publication can be considered. Below I try to explain again why I think the current conclusions are overstated/misleading.

I have to insist on my previous concern that the conclusions are overstated and simply not accurate if we look at the wide heterogeneity of sciatica. To state that discectomy is an option for people with sciatica is not correct, the findings suggest it seems an option for people with sciatica with a surgical indication. As I pointed out before, the vast majority of patients with sciatica will not have any surgical indication and would therefore simply never be considered for these studies (1. It would be unethical and 2. surgeons would not operate if there is no structural correlate (e.g., disc herniation) or clear clinical indication to operate).

The authors point out in their reply to my concern (nr 27) that their review only included patients with an MRI confirmed disc herniation. However, as the authors must be aware, disc herniations are very common, even in healthy people. The mere presence of a disc hernation is not the only deciding factor for surgery. So even if the studies do not report failed conservative management (and thank you to the authors for checking this) or how exactly the surgeons decided it was ok to offer surgery, the population in the studies must be a very select population, which have 1) disc herniation and 2) a clinical picture that makes surgeons offer surgery (e.g., severe or progressive weakness; uncontrollable pain, please refer to international guidelines on this such as NICE). So, the patients in these studies do not only have a disc herniation, but they also do have a surgical indication. And in fact not very many patients with sciatica have this. I do not think that the authors made this clear enough in the discussion section.

Unfortunately, I feel strongly about this point as it is misleading to suggest that discectomy is an option for (all) patients with sciatica. Currently, the abstract/title/conclusion in discussion implies that. There is a responsibility of researchers to be accurate, as patients more and more inform themselves in the medical literature, and the current conclusions leave much room for misinterpretation (also for clinicians).

I suggest to carefully discuss this issue in the discussion section (e.g., the select group of patients likely included in these studies) and also change the title and conclusions in both discussion and abstract to more accurately reflect the patient population (rather than just saying 'sciatica'). Please also amend the 'what this paper adds' section. The conclusion could read as follows:

Discectomy may be an option for people with sciatica with a surgical indication, who feel that the rapid relief offered by discectomy outweighs the risks and costs associated with surgery.

Methods section on subgroup and and exploratory meta-regression: please specify in this part which ones were pre-planned/posthoc analyses.

Typo/grammar page 13: In trials without 'failing non-surgical treatment' in the inclusion criteria, discectomy had larger effects in reducing leg pain at immediate term (MD -19.3, 95% CI-30.4 to -8.2 vs MD -1.2 95% CI-5.5 to 3.1) and improving disability at short term (MD -10.6, 95% CI -14.0 to -7.3 vs MD -1.3, 95% CI -9.8 to 7.3) than trials which only included participants who had failed long term non-surgical treatment.

Also, why do you specify long-term non-surgical treatment in this sentence? In the methods you only stated that you distinguished failed vs non failed conservative treatment with no reference to timing? I do not think you should include only long term non-surgical treatment here as non-surgical treatment failure, but also studies that included patients with failed short term non-surgical treatment as per current guidelines (e.g., NICE recommends surgery consideration after 6 weeks of uncontrollable pain). Also, please label this analysis as a posthoc analysis both in methods and results.

Discussion:
Page 14: "Different from recent reviews, 13 15 16 we included trials conducted in a homogeneous population/surgical procedure/comparator, studies published in English and other languages, 27 33 39 43 47 and new robust trials published recently, 50-53 55 making this review the most comprehensive update on the evidence for the surgical management of sciatica that can provide more informative and nuanced results than the recent network meta-analysis which lumped results across all timepoint into one value."
Please revise this sentence. It is too long. Also, I suggest the authors remain more humble in their writing, e.g., their meta-analysis does indeed provide more nuanced data on certain things (e.g., separate time points), however the network meta-analysis had other findings which were more nuanced than those achieved in this present review (e.g., the network analysis). I simply suggest to remove the last part.

Limitations: "Reporting of non-surgical comparators was generally poor, with most trials failing at describing what types of treatments participants received, who provided these treatments, how they were provided and how much treatment they received."
Please make it clear to the audience that you lumped any kind of non-surgical interventions together and that these were not only poorly reported, but also highly heterogenous.

Supplemental file 20c, d: it seems that the meta-regression on duration of symptoms was done on dichotomized data? This should have been done on continuous data to avoid information loss.

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