

Letters to the Editor

Reply to letter to the editor regarding “Wound drain in lumbar arthrodesis for degenerative disease: an experimental, multicenter, randomized controlled trial”

To the Editor:

I appreciate your interest in the article and thank you for your comments. As for the questions, I have provided the answers.

Regarding commentary 1: Molina et al. state that the SF-36 scores (quality of life) are significantly better in the non-drainage patient group compared to drainage patient group. Moreover, they state that the baseline SF-36 scores (baseline) were not significantly different between the 2 groups. We think, that this statement is not true as the baseline parameters were already significantly different to the disadvantage of drainage patients (SF 36 baseline: drainage group 26.6 – nondrainage group 35.5 ($p=.024$). This significant difference remained at the 30-day SF-36 (SF 36 30 days: drainage group 56.7 – nondrainage group 67.9 ($p=.043$) but was not a new development. If you have a closer look at the increase of the SF-36 scores from baseline to 30 days after operation, it is obvious, that there is no significant difference between the 2 groups (difference drainage group 30.1 – nondrainage group 32.4) which is the point of interest. It should already be clear in the abstract but also in the discussion section that the nondrainage patients have no better quality of life due to the absence of drainage, because these patients had already a better quality of life score preoperatively.

Answer 1: The basal SF36 was not equivalent in both groups; as you mentioned, it was worse in the group with drainage than in the group without drainage, with a p of .024. As expected, the SF36 improved in both groups in the postoperative period (around 30 points). The SF36 of the group without drain improved slightly more than the group with drain (30.1 vs 32.4), with no statistical difference, maintaining a difference of around 30 points [1]. I agree with the comment you made.

Regarding the commentary 2: Another important aspect is that the two groups may not have been equal in terms of surgery. Surgery time was 30 minutes longer in the drain group. Although this is not significant, it does suggest that there were differences. Therefore, the authors should list what the number of TLIF- and PLIF-operations were, as

well as the number of levels in the two arms. The authors stated that the median was the same with two segments. But to state the median makes no sense because it is always 2 with 1 to 3 segments, independent of the choice of treatment type. Thus, it is questionable whether the two groups were really comparable in terms of the type of surgery (TLIF or PLIF, number of segments).

Answer 2: The surgery time did not have significant differences between both groups. The complexity of the surgeries based on the diagnoses, number of operated levels, number of decompressed levels, and number of interbody fusions (TLIF-PLIF) performed on the patients in each group was analyzed. No difference was found in any of these variables. More TLIF were performed in the no-drain group: 12 TLIF in the no-drain group and 7 TLIF in the drain group (p value=.571). Regarding the number of arthrodesis levels in the group without drainage: 33 patients with 1 arthrodesis level, 12 with 2 arthrodesis levels, and 3 with 3 arthrodesis levels. For the group with drainage: 30 patients with 1 arthrodesis level, 11 with 2 arthrodesis levels, and 4 with 3 arthrodesis levels. There was no statistical difference found between the groups (p value=.482).

Regarding the commentary 3: Previous spine surgery is an important exclusion criterion and represents a considerable preselection. We think, that this statement should be included in the abstract to inform the reader properly about the selection of patients.

Answer 3: Previous surgery seems to be a factor that could affect the safety of the patients in our study; for this reason, we defined it as an exclusion criterion.

The information regarding the inclusion criteria of “primary surgery” is specified in the Abstract.

Future studies will be able to evaluate whether or not previous surgery affects the use of drainage.

Regarding commentary 4: Furthermore, it is not described whether there was a fixed scheme regarding the drainage or not. When were the drains pulled out? Molina et al. made a statement of blood loss after 2.61 days. So obviously the drains were pulled out after 2 to 3 days. But what were the criteria? If the drainage always got pulled out after 3 days, it is also not surprising that the length of stay was 1 day higher than in the nondrainage group.

Answer 4: There was no protocol for drain removal. There is no consensus in the literature on this point. It is highly variable between doctors from the same hospital and doctors from different centers. Some doctors remove the drain on the first day regardless of the blood volume; some

have variable drainage removal criteria (<50 cc/ 12 h; <100 cc/12 h; (<50 cc/ 24 h; <100 cc/24 h, etc.) and many do not have defined criteria.

Regarding the fact that the time in which the drain is removed influences the medical discharge, I agree with you. A patient who removes the drain on the third day undoubtedly influences a later discharge. Therefore, not using a drain was related to a shorter hospital stay. The important thing is that this earlier discharge is safe for the patient and is not associated with a higher frequency of complications.

Regarding commentary 5: Another point of concern is that it was clear before the operation into which group the patients were randomized. Thus, the surgeons knew who would get a drain and who would not. This information may have influenced the type of surgical technique. At least this is another limitation of the study.

Answer 5: As it says in the Surgical Procedure section, the randomization of the patients was done 1 hour before starting the surgery (before the incision) using REDCap. The planned surgical technique was not changed in any patient after randomization.

Thank you for your interest in the study. I hope I have addressed your concerns and answered your questions.

Acknowledgments

We would like to thank Dr Alexander Hammer and Dr. Klaus John Schnake for their interest and perceptive insights concerning our study. In your letter, you raise interesting points of view that we will address in this response.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Reference

- [1] Molina M, Torres R, Castro M, Gonzáles L, Weissmann K, Martinez M, et al. Wound drain in lumbar arthrodesis for degenerative disease: an experimental, multicenter, randomized controlled trial. *Spine J* 2023;23(4):473–83. <https://doi.org/10.1016/j.spinee.2022.12.002>.

Marcelo Molina, MD*

Department of Orthopedic Surgery, Spine Center, Clínica Alemana, Av. Vitacura 5951, Santiago, Chile Universidad Finis Terrae, School of Medicine, Santiago, Chile

**Corresponding author. Department of Orthopedic Surgery, Spine Center, Clínica Alemana, Av. Vitacura 5951, Santiago, Chile.*

E-mail address: marcelomolinasal@gmail.com

FDA device/drug status: Not applicable.

Author disclosures: *MM*: Nothing to disclose.

Marcelo Molina, Ramón Torres, Magdalena Castro, Lucio Gonzáles, Karen Weissmann, Maripaz Martinez, Marcos Ganga, Roberto Postigo. *Spine J* 2023 Apr;23(4):473-483. doi: 10.1016/j.spinee.2022.12.002. Epub 2022 Dec 10.

<https://doi.org/10.1016/j.spinee.2023.09.031>