Peer Review File

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Reviewer A

The authors describe a technique if ITM in anterior and lateral lumbar fusion procedure. This is a sweet and elegant technique but unfortunately nothing novel.

Reply: We thank reviewer A for their comments. We are not trying to present a novel technique but want to highlight via this Technical Note something unique that to our knowledge has not been presented in the literature previously.

Also, it is inappropriate for them to state that lateral and anterior fusion procedures are more popular than posterior procedures, which are still way more commonly performed.

Reply: We had no intention to state that ALSS and LLSS are more popular than posterior approaches but wanted highlight that the number of these procedures is increasing year by year. We have adjusted our statement in the revised manuscript in order to make it clearer.

They describe a good reduction in pain scores but there is nothing to compare with to show that this technique has any benefit over IV morphine. Also they don't mention any case of abdominal distension, which is the most common side effect of ITM injection. This article is not suitable for publication in this journal in its current form. May be a control arm would strengthen the validity of the technique.

Reply: Regarding the distension, ileus is a common complication after ALSS with or without ITM but in our reported cases we did not see an increase in prevalence in comparison with patients not treated in ITM. It is well known in the literature and that ITM significantly improves post-op pain. Inspite of this we agree with the reviewer that a control arm is a good idea and this will be our next step for running a prospective study. In the meantime, we would like to publish as a Technical note.

Reviewer B

The Authors present an interesting technical note describing an intra-operative technique for injection into the dural sac via the Anterior and Lateral approaches to the Lumbar Spine on 24 patients'.

The procedure, however simple, appears to be described for the first time. Personally, I consider the pathway to the dural sac created by the ALIF approach potentially valid, but I do not think that the path created by an LLSS approach is sufficiently safe. Gently bending a 26 gauge needle without a direct view of the injection site could be risky and appears not so replicable.

Reply: We thank reviewer B for their comments. In our LLSS we perform the procedure with direct vision of the disc space. We clean the posterior part of the disc space all the way to the PLL and direct our Spinal Needle under magnification of loupes. We have clarified this in the manuscript.

Furthermore, the method of verifying the absence or presence of CSF fistula is not clearly described. Do the authors refer to intraoperative vision or postoperative clinical examination or instrumental exams?
The retroperitoneal space would allow the slow formation of even large collections of CSF. A small CSF fistula may also not be clinically symptomatic in the early postoperative days.

Reply: In order to verify the presence or absence of CSF fistula we rely on postoperative symptoms and signs. We perform Ulrtrasound of the Abdomen and Pelvis for every patient prior to discharge to exclude any collection (Manuscript amended to reflect this). Patients remain in hospital for 2-4 days and are followed up 4 weeks, 6 months and 12 months and there was no evidence of any symptoms or signs to suggest CSF fistula.

In conclusion, I think that the described procedure could be of scientific interest if presented as a clinical study by expanding the number of patients, analyzing the

clinical data (statistically) and reporting complications with a minimum of follow up. I would try to rule out LLSS procedures. Focus on ALSS procedures.

Reviewer C

There are two major concerns with this study as written:

1. There is no comparison cohort, and yet there are primary outcome variables of VAS and PCA requirement. Without a comparator group, these outcomes are meaningless. In a single cohort small study, outcomes should be related to safety/technical feasibility, and therefore the authors should stress complication rate as their primary outcome in the Methods/Results, as they appropriately do the in Discussion.

Reply: We thank reviewer C for their comments. We agree and have changed the primary outcomes to be complications.

2. That said, "PCA requirement" alone is also a poor variable. Narcotic consumption in the immediate in-hospital post-operative period in MME is easily calculated from EMR, as is other analgesic and symptomatic prn medication consumption, and should therefore be reported if the authors want to make an argument about postoperative pain reduction.

Reply: We agree that PCA requirement is a poor viable and have amended this in the revised manuscript to be total narcotic consumption. In our protocol when we give ITM via ALLSS/LLSS the patients received no additional Morphine infusions. They are started day 2 on oral pain medication.

Minor notes:

1. Page 5, Line 5: "roll" is misspelled "role"

Reply: Corrected in manuscript

2. The "Injection Technique" section should be a sub-section of Methods

Reply: Corrected in manuscript

With this being said I commend the authors on a straightforward, useful technical note. With the above considerations regarding framing/reporting of their findings, I would recommend this study for publication.

Reply: Although there is no comparison cohort it is well known in the literature 2. We have a protocol when we give ITM the patients received no additional Morphine injections and we start them on day 2 on oral pain medication. Total Narcotic Consumption reduced (instead of PCA).