Author's response to reviews

Title: Femoral nerve block - sciatic nerve block vs. femoral nerve block-local infiltration analgesia for total knee arthroplasty: a randomized controlled trial

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Author's response to reviews: see over
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Marielette Costoy
Journal Editorial Office
BMC Anesthesiology

Dear Marielette Costoy

Thank you for returning our manuscript with comments from the three reviewers. My co-authors and I have read through the comments very carefully and have revised the entire text to present our findings more clearly. For the reviewers’ convenience, all revised portions have been highlighted in a bolded font.

We believe that the amendments to the revised version have addressed the reviewers’ concerns, and hope you will find the revised manuscript acceptable for BMC Anesthesiology. If you have further comments or concerns, my co-authors and I will be pleased to address them as soon as possible.

I look forward to hearing from you.

Yours sincerely,

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Responses to the comments from Dr. Devendra Mahadevan

Overall assessment:

The study does ask a reasonable question that may have clinical implications. It has been set out the right way. However, I think there is a serious flaw in the sample size estimation. The calculations suggested a group size of 28 (14 per group). I have tried to calculate it myself and with the same parameters used (effect size 0.25 (for NRS I assumed), beta error 0.2, alpha error 0.05) my calculations show that 128 participants are required. With such an underpowered study, the results cannot be deemed as valid.

Authors’ Response

Thank you for your advice.

Our hypothesis for this RCT was that there would be no difference in NRS score changes between the groups, as presented in line 247 (in the outcome measures section of materials and method) of the original manuscript. This was our primary end point. We used repeated measure analysis of variance (ANOVA) to compare the changes of NRS scores. We used G power 3 to determine the sample size for this statistical method.
In this process, we set the alpha error at 0.05, beta error at 0.2, and effect size at 0.25, and the required sample size for detecting a significant difference between the groups was calculated to be 28. We have attached our calculations from this process as reference data.

We compared the differences in NRS scores at each time point as a second end point. The required sample size for this second end point using the t test was 128, as the reviewer had calculated. We have also attached our calculations for this process as reference data.

We apologize for our hypothesis in this RCT being obscure. We included the hypothesis for this RCT in the introduction (line 116-117) to present it more clearly, as also recommended by another reviewer.

Comments:

Abstract: No issues

Background:

Line 139: Remove ‘used for pulmonary embolism prevention’ as anticoagulation indication is more than for PE.
Authors’ Response

Thank you for your advice. We deleted this portion.

Line 150: ‘comparing the analgesic effect of LIA with that of SNB’ – there has been another RCT looking at this but has not been cited (Combined femoral and sciatic nerve block vs combined femoral and periarticular infiltration in total knee arthroplasty: a randomized controlled trial. J Arthroplasty. 2012 Dec;27(10):1806-11).

Authors’ Response

Thank you for your advice. We cited this reference (reference No. 44) and compared its results with those of our study in lines 327-332.

Materials and Methods:

Line 189 onwards: We need to know whether all patients underwent patella resurfacing or not as this will influence the results. What was the criteria used to decide on who was resurfaced or not?

Statistics: as mentioned above.

Results:
We need numbers who had resurfacing.

Authors’ Response

Thank you for your advice. We performed patella resurfacing when the cartilage damage of the patella was severe and the damage reached to the subchondral bone (International Cartilage Repair Society Grade IV). In this study, we did not perform patellar resurfacing in all patients. We added the description about resurfacing in lines 154-156.

Line 310 onwards: I would prefer if the authors presented the raw NRS data.

Need to know the spread and SD etc.

Authors’ Response

Thank you for your advice. We presented the raw NRS data in a new table (Table 2). As the NRS score is a discrete value, the data at each time point were analyzed by the Mann-Whitney U test. Although this test is nonparametric, the repeated-measures ANOVA is parametric and the mean and SD are used in the figure; therefore, we used the mean and SD for the raw NRS data.

There was no mention of post-operative Diclofenac consumption – this is useful
information as it is an indirect way of assessing pain – this needs to be included.

There was no data on fentanyl consumption intra-operatively. Again, this is useful as it will show whether the addition of SNB provided any pre-emptive analgesic effect.

*Authors’ Response*

Thank you for your advice. Post-operative diclofenac consumption is mentioned in Table 2 as the "dose of analgesic (mg)" in the original manuscript. However, this description is unclear, as you have pointed out. Additionally, we included incorrect data. We apologize for this mistake. We counted the number of diclofenac doses used. We intended to present the number of diclofenac multiplied by 25, but we multiplied the number of diclofenac by 15 by mistake. We changed the description to "dose of diclofenac (mg)" and have now included the correct data in Table 3 of our new manuscript. Please accept our apologies for our mistake.

We used 50 µg of fentanyl in the course of induction as stated in line 184 of the original manuscript.
We also stated that "no additional narcotics or analgesics were administered during the operation." in line 188 of the original manuscript. This expression is obscure, as you have pointed out. We rewrote this portion as "no additional narcotics or analgesics including fentanyl were administered during the operation." We included this sentence in line 152 of our new manuscript.

Discussion:

Line 341: The authors mentioned no difference in analgesic requirements, but I could not find this data within the article.

Authors' Response

Thank you for your advice. We described post-operative diclofenac consumption in Table 2 of the original manuscript. We had mentioned "analgesic doses" in line 315 of the original manuscript. However, this was unclear. We now refer to "dose of diclofenac" in line 213, 282 of our new manuscript.

Responses to the comments from Dr. Sachiyuki Tsukada

Major concerns:

1. The periarticular injection (local anesthetic infiltration) of this study included
only ropivacaine and adrenaline. Modern periarticular injection for TKA typically includes opioid and/or corticosteroid in addition to these drugs. Almost all readers consider that the agent of periarticular injection introduce the result of this study that periarticular injection was inferior.

Authors must describe the content of the periarticular injection briefly in the abstract. In the discussion, refer the recently published articles, especially in major Orthopaedic journals, investigating the effectiveness of modern periarticular injection and compare the results with the result of this study. The content of line 367-8 is very insufficient.

Authors’ Response

Thank you for your advice. We described the content of the periarticular injection in the abstract (line 61-65). We referred to recently published articles (reference No. 35-38) and compared the results with those of our study (in line 324-332).

2. The authors must write the way of randomization in detail. Why was the number allocated to two groups completely same in spite of using numbered container method including randomized number?
Authors’ Response

Thank you for your advice. The method was described insufficiently. We had described "numbered cards in an opaque envelope were used" in line 166 of the original manuscript. We rewrote this portion as "In total, 34 numbered cards assigned as L1-L17 and S1-S17 in an opaque envelope were used for random allocation to group" in line 129-131 of our new manuscript.

Minor concerns:

1. The introduction part is too long. But, the introduction part does not include the hypothesis of this RCT.

Authors’ Response

Thank you for your advice. We included the hypothesis of the RCT in the introduction (in line 116-117). We reduced the size of the introduction from 312 words to 225 words.

2. The authors can delete line 129 to 136.

Authors’ Response

Thank you for your advice. We deleted lines 129 to 136.
3. Did the surgeons use the air tourniquet during study period?

Authors' Response

Surgeons used the air tourniquet during the study period.

4. Reviewer doesn’t know Statcel. Is this software valid for scientific article?

Authors' Response

Statcel is the statistical package for Microsoft Excel. We re-evaluated our data using SPSS statistical software and rewrote the results accordingly.

5. The reviewer recommends authors to write registry number of RCT in M&M part.

Authors' Response

Thank you for your advice. We included the registry number of the RCT in lines 124-125 of the new manuscript.

6. In RCT, the primary outcome should be one. The reviewer recommends to use the area under the curve of NRS instead of multiple assessment.

Authors' Response
Thank you for your advice. The concept of the area under the curve of the NRS score is clear-cut. We also calculated and compared the area under the curve of the NRS scores using SPSS. There was a significant difference between the two groups. We stated this at lines 239 and line 278-280. However, if we change the primary outcome from a comparison of changes in NRS scores between groups L and S to a comparison of the area under the curve of the NRS scores, a sample size of 134 would be needed for Mann-Whitney statistical analysis. Changing the sample size of the study would be difficult at this stage. We would thus like to include the change in the area under the curve of the NRS scores as a secondary outcome. We would change the primary outcome per the reviewer’s recommendation if our original sample size would be sufficient.

7. The discussion part is too long. Authors should decrease it fewer than half of the primary manuscript.

Authors’ Response
Thank you for your advice. We removed more than half of the discussion. The number of words in the discussion part of the primary manuscript was 1079. The number of words in the revised manuscript is 539.

8. Refer more recently published studies.

Authors’ Response

Thank you for your advice. We referred to recently published studies. We added 18 recently published references.

9. Please focus on the content of periarticular injection. Not the dose of local anesthetic. The problem is that the periarticular injection you used did not include opioid and corticosteroid.

Authors’ Response

Thank you for your advice. We have focused on the contents of the periarticular injection in lines 322-332 of our revised manuscript.

Responses to the comments from Dr. Patricia Lavand’homme

The authors compare the analgesic effect of two loco-regionale techniques, continuous femoral nerve block-sciatic nerve block or continuous femoral nerve
block-LIA, for pain relief after knee arthroplasty. The authors report better pain relief with sciatic nerve block association but only during the first 12 hours post-surgery. No impact was detected on other outcomes like LOS, mobility.

Introduction: clear

Methods:

- remifentanil was used at various infusion rate "as needed": please comment.

Does it mean that some blocks were less effective to ensure intraoperative anesthesia? Was it to control blood pressure?

Authors’ Response

Thank you for your advice. Intraoperative remifentanil was used to obtain stable blood pressure and heart rate during operation. As remifentanil administration was stopped before skin closure and radiographs were obtained postoperatively in the operating room, at least 30 min had passed from the cessation of remifentanil; thus, the associated effects had almost disappeared by the time the patients exited the operating room.

- were the patients included in a fast-track recovery programme?

Authors’ Response
Thank you for your advice. The patients were not included in a fast-track recovery program.

- LIA administration may be highly variable; the authors should comment on the volume of local anesthetic used

Authors’ Response

Thank you for your advice. We described the volume of local anesthetic used in lines 370-378 of the previous manuscript. We rewrote it in lines 189-197 of the revised manuscript. However, this was insufficient. Therefore, we also added new comments regarding the content of the anesthetic in lines 61-65 in the abstract.

- among the various outcomes, it would have been interesting to question patients about satisfaction regarding their postoperative analgesia; also interesting to assess longer term impact of the two different techniques (e.g. 3 or 6 weeks outcomes)

Authors’ Response

Thank you for your advice.
We assessed patient satisfaction relating to pain control on the third postoperative day, consisting of a five-point scale (in line 230-233, 281-282). The assessment of longer-term impact of the two different techniques is interesting. However, our study started in October 2012, i.e., approximately 3 years have passed since the initiation; therefore collection of longer-term data would be difficult.

In particular, the accuracy of memories from 3 years ago is unreliable, and because of the long interval, many patients are expected to drop out in the course of the investigation. We apologize for not considering a longer-term investigation at the time the study was initiated.

- statistical analysis is provided

Results: very clear and not surprising

Discussion: correct but too long

Authors’ Response

Thank you for your advice. We removed more than half of the discussion. The number of words of discussion in the original manuscript was 1079. The number of words in the revised manuscript is 539.