Author's response to reviews

Title: Transversus abdominis-plane block versus local anesthetic infiltration in lower abdominal surgery: a meta-analysis of randomized controlled trials

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Author's response to reviews: see over
COVER LETTER

Thank you for your letter and the reviewers’ comments concerning our manuscript entitled “Transversus Abdominis Plane Block or Local Anesthetic Infiltration? A Meta-Analysis of Randomized Controlled Trials” (MS: 5874865781248411). Those comments are all valuable and very helpful for revising and improving our paper, as well as the important guiding significance to our researches. The main changes are: 1. We removed two laparoscopic surgery articles, as one reviewer mentioned it is hard to compare the pain magnitude between laparoscopic surgery and open abdominal surgery. 2. The language was modified by Edanz. 3. The tables are rearranged. 4. The abstract, background and discussion are rewritten, and the modifications are in red in the latest version. Below is the point-by-point response. We would be grateful if you would consider our manuscript for publication. And please don’t hesitate to contact us if you have any more questions. Thank you very much for your consideration.

Sincerely,

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Reviewer's report

Title: Transversus Abdominis Plane Block or Local Anesthetic Infiltration? A Meta-Analysis of Randomized Controlled Trials

Version: 3

Date: 29 April 2014

Reviewer: Dan Benhamou

Reviewer's report:

This meta-analysis is interesting, although not providing much novelty. I believe that at least some changes would be necessary before considering acceptance. Although there are relatively few studies in total, one additional analysis would be to evaluate the type of local infiltration. As shown for example by Liu et al (JAMA 2006), local infiltration is a generic word for which there are several techniques. Recently, several authors have shown that preperitoneal infiltration provides a better effect than other sites of infiltration. The authors should try to discriminate these two situations.

Reply: The studies included in our study are all comparing local anesthesia injection into skin and subcutaneous tissue at surgical incisions with TAP. No study with deep fascia or preperitoneal infiltration was included. The information has been corrected in the last paragraph of introduction in the revised manuscript.

It should also be stated that only single shot injections are studied.

Reply: Already corrected in the last paragraph of introduction in the revised manuscript.

Additional questions and comments Summary

P2, L51: the present study does not compare the two techniques only after lower abdominal surgery? Please rephrase.

Reply: Already corrected in the revised manuscript.

P2, L53: the study was not aimed at determining the efficacy of TAP block but to compare its efficacy with that provided by LAI. Please rephrase.

Reply: Already corrected in the revised manuscript.

P2, L58-61: details of the statistical study are unnecessary in the abstract section.

Reply: Already corrected in the revised manuscript.

P2, L68: sat???
Reply: It should be at, typo. Already corrected in the revised manuscript.

Introduction

P3, L87: Kuppuvelumani et al

Reply: Already corrected in the revised manuscript.

P5, L128: abdominal surgery or lower abdominal surgery?

Reply: Lower abdominal surgery. Already corrected in the revised manuscript.

P5, L129; single shot studies

Reply: Already corrected in the revised manuscript.

P6, L143 and P6, L150-152: these sentences seem similar. Should they be repeated?

Reply: Yes. L143 talks about the eligibility of the articles, while L150 is about data extraction. Both need two authors evaluate the quality of/extract the data from the articles under the supervision of a senior author, so the sentences look similar.

P7, L179: Jadad

Reply: Already corrected in the revised manuscript.

Results

P8, L186: VAS scores at 2 hours, not 24h

Reply: Already corrected in the revised manuscript.

P9, L215 and L221: was the change significant or was it a trend? If it was significant, only state that and delete the word “trend.”

Reply: It was statistically significant. Already deleted the word “trend” in the revised manuscript.

P10, L239: are summarized

Reply: Already corrected in the revised manuscript.

P10, L242: funnel plot is shown

Reply: Already attached as additional materials in the revised manuscript.

P11, L258: what means handy?

Reply: We meant to say LAI is a convenient method to perform. Already changed handy to convenient in the revised manuscript.
Discussion

P13, L314-316: please delete the last sentence, which is out of the scope of the study.

Reply: Already deleted in the revised manuscript.

Table 1 and 2 are almost impossible to read. Please change the format.

Reply: Already rearranged in the revised manuscript.

The readers would be more interested by the Forest plots rather than by the funnel plots and the figures showing methodological issues. Please place as additional files Figures 2, 3 and 4 and place in the paper itself the figures that are presently as supplementary files.

Reply: Already rearranged the funnel plots and methodological issues to the supplementary file in the revised manuscript.

References

Please verify all abbreviations for journal titles and use official and internationally accepted abbreviations

Reply: Already verified according to the request of the journal in the revised manuscript.

Reviewer's report

Title: Transversus Abdominis Plane Block or Local Anesthetic Infiltration? A Meta-Analysis of Randomized Controlled Trials

Version: 3

Date: 14 April 2014

Reviewer: Nicholas Ventham

Reviewer's report:

Many thanks for submitting this meta-analysis.

The review is largely well written (especially methods and results are very clear), and addresses the question. The statistics are logical, and well performed. The figures are clear and informative. I also think the bias assessment used is a robust methodology,
although i would disagree on some of the scores given (especially those in the Cochrane bias plot). I sympathise with the authors of this study regarding contacting primary authors for information - this is difficult to do, and authors rarely get back to you.

I do however have some major methodological concerns, which may be difficult to address.

My major problem with this article is the inclusion of very different types of surgery. Although the authors do mention this (briefly) in the discussion, this is a highly confounding factor with this type of review. The trials include vastly different surgery types (the pain following lap chole is unlikely to be similar type or magnitude to pain following gyane surgery and inguinal hernia surgery) - no attempt has been made to address these differing types and routes of surgery (open/laparoscopic)

Reply: According to the VAS scores reported in the included articles, the pain magnitudes of laparoscopic cholecystectomy and gynecological surgery are similar. The difference can be due to individual difference, since it is hard to compare the pain magnitude between laparoscopic surgery and open abdominal surgery. As is mentioned, TAP is mainly used for lower abdominal surgeries, and the two upper abdominal surgeries (laparoscopic surgeries) don't have enough data to do subgroup analysis, we decided to deleted these two laparoscopic cholecystectomy articles and focus on the effect of TAP block on post-op analgesia of lower abdominal surgeries.

Similarly, another major issue is the type of TAP block/local anaesthetic agent used, and the INTRA-operative analgesics, and additional post-op analgesics (e.g. paracetamol, NSAIDs) are not sufficiently accounted for (difficult within the analysis, but should be discussed at least)

Reply: Already discussed in the revised manuscript.

I am concerned by the (lack of) primary outcome. If VAS score was decided to be the primary outcome, (not clear in abstract, methods, results - I would suggest that 24 hour morphine consumption is more objective parameter to use) this is not a good choice, it is largely subjective, with high intra-subject variability. Also, as the authors briefly mention, the effect size is hard to appreciate - do the authors think that a 0.67 point reduction in VAS at 24 hours (given the varying types of operation included), with high intra-subject variability is sufficient to recommend one treatment modality over another. There are published 'VAS effect sizes' deemed to be clinically significant, which could be referenced.

Reply: VAS score was the primary outcome, as we put it first in the results of the article. We admit that VAS score is individually different, but it is the most widely used pain score, which objectively measures the subjective pain magnitude. And the 'VAS effect size' is discussed in the revised manuscript.
Major compulsory revisions

The first paragraphs of the introduction and discussion are poor and should be re-written.

Reply: These parts are re-written in the revised manuscript.

Do all included papers publish their data in the form of mean and sd? if not how has this been incorporated into analyses, were studies excluded if data was not in this format.

Reply: Not all of the studies reported the data in the form of mean and SD. If the study reported the data with mean and SE, it was converted to SD using the formula SD=SE/Vn. When we contacted the corresponding authors, we requested them to report the original data in the form of mean ± SD.

Please include the funnel plots (I could not see this in the online/supp materials)

Reply: The funnel plots are included in the revised supp material.

Are there any other relevant indices of recovery - time to mobilisation/diet resumption/discharge time (all difficult to analyse statistically) - that could have been analysed?

Reply: These indices are beneficial and patient-oriented, but none of the relevant studies includes relevant information.

Were there any complications from either TAP block or local anaesthetic infiltration?

Reply: TAP block is not without risk. Vascular, visceral and nerve injuries, and catheter breakage, have been reported. Vascular and nerve injuries, toxicity from an excessively high concentration of the drug in the blood are the potential risks of LAI. But none was reported in the studies included. Discussion about the complications has been added in the revised manuscript. And the complications are discussed in the revised manuscript.

Minor issues

The use of 'handy' in the discussion is colloquial and should be changed (also seen in a different font)

Reply: Already corrected in the revised manuscript.

Level of interest: An article of limited interest

Quality of written English: Not suitable for publication unless extensively edited

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests: No competing interests.