Peer Review File

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Reviewer: A

Comments to the authors:

Congratulation on this well written and very informative descriptive literature review. This is a very relevant topic and I would wish to see it published.

My suggestions towards the manuscript changes are:

1. Main focus should be made on methodology section with presenting a well defined research question, methods used for literature search, such as search terms, inclusion and exclusion criteria and grading. Please adhere to one of the commonly used and evidence-based checklists for reporting of systematic reviews such as eg. PRISMA. Include the flow diagram and checklist in your submission.
   The current review is not a systematic review, nor does it include a meta-analysis. The literature is far too sparse and heterogenous for a meaningful meta-analysis. In accordance with the Editor's comments (see below, a 'methods' section and a PRISMA flowchart is included in the revised version of the manuscript).

2. I find pathophysiological aspects and laparoscopic technique paragraphs well written but irrelevant for the purpose of a literature review. With good photographs or a video, I'm sure those could be published as a separate manuscript eg. How I do it article.
   The review is narrative in nature and the target was general non-academic surgeons with no comprehensive pathophysiological knowledge and familiarity of surgical techniques for the pain-operations. Therefore, we think the pathophysiological and surgical technique have a role in the review and we kindly ask the Editor to keep the two mini-chapter. We can of course reduce the words if the Editor think it would be appropriate?

3. Inclusion and comparison to other surgical and non-surgical methods of treatment of neuropathic pain after groin hernia repair in order for the readers to have boarder view on the outcome of specific methods and the role of laparoscopic neurectomies in association to alternatives. I would suggest boarder search and literature review. Otherwise, please include a paragraph describing this significant weakness of the presented review.
   We are not sure that we understand this comment and we do not know what is meant

by “boarder search”. However, a paragraph about study limitations has been added to the discussion section.

Reviewer B

Comments to the authors:

Nice study, however, to improve the value some major revision should be done:

1. For more clarity the abstract should be well structured according to usual rules in “ Introduction, Methods……..”
   The reviewer wants the abstract to be structured, however that is in violation with the journal’s requirements. The journal’s requirement is that the abstract should be unstructured and consist of 200-350 words max. The abstract structure has not been altered.

2. Regarding the title I would recommend “Laparo-endoscopic treatment……). “Laparoscopic” means “transabdominal approach” and “Endoscopic” means “complete extraperitoneal”
   In accordance with the editorial comments (see below), the title is maintained in the revised version of the manuscript.

3. The literature search should be precisely described, e.g. which searching machines and search terms are used, how many items were found and how many out of that total number were suitable for analysis. The authors analysed 6 papers only. According to the guidelines published by the HerniaSurge Group there are existing 7papers related to Triple-Neurectomy and 18 papers related to tailored neurectomy. How is this discrepancy explained?
   See point 1, Reviewer A
   The authors can assure that there exist more than the 25 papers as listed by the HerniaSurg Group (1), however the authors chose to only describe prospective studies employing laparoscopic technique and not open technique (as this fits the journal’s scope). This has been added to the methods section.
   .
4. The authors should more clearly differentiate between patients suffering from post - operative chronic pain plus a recurrence and patients suffering from pain but without any recurrence.
   The included studies all take recurrence into account when selecting patients for operation and patients with recurrences are excluded. This was already in the manuscript.

5. The role of preoperative pain is not discussed. The authors should do their literature overview with focus on this problem, too. In my experience the vast majority of chronic pain patients suffer from pain already before surgery, but the pain may be not caused by the hernia. Therefore, indication for surgery should be made very carefully. The same is true also in pain patients after the operation. The authors should more emphasize that maximal caution is demanded when indicating risky re-operations. Neurologic and psychologic investigations are absolutely necessary before operative treatment. The literature should be checked if all patients suffering from chronic pain had these investigations.
   A revision has been made according to the comments by the reviewer (Discussion section).

6. It must be emphasized that neurectomy, if it is necessary, must be done proximal to the site of neuropathy, this means very close to the root of the nerves when passing the spine either endoscopically, laparoscopically or open retroperitoneally.
   In our manuscript we write: "but preferably as distal as possible to minimize the risk of laxity of the flank muscles". This has been changed to "but, based on weak evidence, preferably as distal as possible to minimize the risk of laxity of the flank muscles".

7. Why removal of the mesh when it is possible to detach all the adhesions to the abdominal wall incl. all of the nerval and vascular structures. The mesh per se does not produce pain, but its removal will result in a large peritoneal defect with a lot of problems. This should be discussed. In the revised version of the manuscript we have elaborated on the role of the mesh *per se* as the source of chronic pain

Reviewer C

The authors have made a narrative review of laparoscopic treatment for chronic post inguinal herniotomy pain. As such the subject is relevant due to the high number of clinical cases and the lack of evidence as to what treatment modalities to choose.

However, the current paper does not add anything new to the existing literature, and the conclusions and recommendations are also found in the papers cited.

Comments:
Introduction:

1. Page 3 line 42: the authors write "Preliminary result have suggested" and end up

stating, “the evidence is weak and the results not uniform”. Which one is it?..
Preliminary results have been changed to ’early results’ in the revised version of the manuscript.

2. The description of the assumed pathophysiology behind chronic post inguinal herniotomy pain is very superficial focusing on nerve injury, without any mentioning of related psycho-social factors, the many investigations into the nociceptive system, genetics, inflammation etc.
   See point 2, Reviewer A.

3. The authors focus on so-called nociceptive pain while acknowledging that differentiation between nociceptive and neuropathic pain is often impossible, and especially there is no discussion of why such a distinction may be important (we have no mechanism based treatments)…
   We thank the reviewer for this important remark and have accordingly revised the manuscript (Discussion section).

4. QST is described as a meticulous procedure, forgetting that QST spans from a full battery taking hours, to a simple pinprick or pressure test, very much clinically feasible and optimal for follow-up. The questionnaires have poor specificity and sensitivity, and again is the relevance of distinction between pain types not described.
   The section regarding QST has been revised in the revised version of the manuscript.

Surgical technique description section:

5. -The section lacks information that nerve anatomy is highly varied, and don’t always presents as simple as stated by the authors.

The manuscript is revised according to the reviewer’s comment.

6. There is no description of methodology.
   - What were the inclusion criteria, how were included papers identified? A database search strategy should be supplied or at least that the papers were based upon the authors knowledge, reference search etc.
   - What papers were excluded and why?

At least one paper (Bjurstrom et al Pain Practice 2016) is missing.

Search strategy and PRIS MA flow-chart has been added to the manuscript.

The Bjurstrom et al 2016 paper published in Pain Practice titled: “Cerebrospinal Fluid Cytokines and Neurotrophic Factors in Human Chronic Pain Populations: A Comprehensive Review” is a review and has not been included according to our search strategy.

7. Papers: the authors summarize the papers, and as such nothing is wrong, but id adds nothing, and if anything the whole section should be omitted and only a discussion left.
   See reviewer A.