Peer review file

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Reviewer A

Comment 1: You show a retrospective study on a cohort of patients undergoing xiphoidectomy for xiphodynia. Your series is well documented with a complete follow up concerning the main outcome, i.e. pre- and postoperative pain using NRS. The illustration of the xiphoidectomy technique is of good quality and clear. Even if a comparative study would be of more interest (comparing different treatment modalities) - which you state in the discussion -, it is the largest series up to now.
Reply 1: Thank you for reviewing the manuscript and your comments.
Changes in the text 1: No changes were made.

Reviewer B

Comment 1: Your handling of pain scores is problematic. Your postop scores are not obtained at specific time intervals after surgery (ie at 6 months, 1 year) but at a specific date (March 2020), this is problematic as it would result in postop pain scores being obtained at different postop time period for each patient. Please revise to show post op pain scores at specific time intervals and re-perform statistical analysis comparing each postop interval with preop.
Answer 1: We agree that postop pain scores at specific time intervals after surgery would be of higher value. Unfortunately, due to the retrospective study design, these pain scores are not available at specific time intervals. We gathered postoperative pain scores by telephone consultation in March 2020. We added this as a limitation of the current study in the discussion section.
Changes in the text 1: See page 10, line 217-219.

Comment 2: You state, “We found no correlation between treatment outcomes and clinical characteristics including amongst others xiphisternal angle and xiphoid

length". Such a statement without showing the full analysis is not helpful. What was the primary endpoint, any symptoms at all (which would be n=5), lack of pain improvement (which would be n=2)? What statistical analysis did you use to assess this "correlation"? Either way, your number of events and total number of patients are too small to perform any meaningful statistical analysis. I would recommend showing this full analysis or deleting this statement all together.
Answer 2: The primary endpoint was the reduction in postoperative pain score. We agree that the number of patients is too small to identify any correlations and subsequently deleted this statement.
Changes in the text 2: See page 8, line 161.

Comment 3: One patient complained of dyspnea after surgery, please explain why this would be.
Answer 3: 1. The dyspnea was already present preoperatively and most probably has a different etiology. A statement regarding this issue was added to the Results section for clarification.
Changes in the text 3: See page 8, lines 155-156.

Comment 4: Your median length of stay is 1 day. Can you comment on whether this procedure can be performed as outpatient? This will cut down costs.
Answer 4: Thank you for your good comment. The procedure might indeed be performed in an outpatient setting and have added this to the discussion section.
Changes in the text 4: See page 10, lines 205-205.

Reviewer C

Comment 1: Overall the manuscript is well written. The concept of refractory xiphoid pain which may be considered a form of costochondritis is not common. As stated by authors, conservative management with NSAIDS or local blocks more common. I think this series would be helpful for surgeons to be aware of such a problem, but patient selection is obviously important and not well described here. Perhaps the authors can convey how long conservative management lasted (on average) before going to surgery? Aside from NSAIDS, did the authors ever consider neuropathic type

treatment with Gabapentin or Lyrica. Are there any patients that the authors excluded that we can learn? As from angulation, but other radiographic findings were the authors looking for on anatomic imaging.
Answer 1: First of all, thank you for your comprehensive review of our manuscript and the comments raised.
We fully agree that proper patient selection for xiphoidectomy is of utter importance. Though, given the small sample size in the present study, we deferred from recommendations on how to select patients for xiphoidectomy, such as recommendations based on xiphisternal angle. Moreover, the duration of conservative management differed between patients and was in most patients several months or even years. This has been added in the results section.
Regarding your question on neuropathic type treatment, we did not consider using such a pain treatment regimen, however, may be worth investigating in the future.
Changes in the text 1: See Page 7, lines 144-145.

Comment 2: Patients 1,2,12 have preoperative pain scores that were low <=4. Why was the operation offered for these patients?
Answer 2: Even though preoperative pain scores in patients 1,2, and 12 were relatively low, these concerned pain scores following conservative treatment and hampered daily patients' life. Given the primary goal to fully relief pain, further treatment in terms of xiphoidectomy was opted. Nevertheless, despite such "good" results were shown for xiphoidectomy based on pre- and postoperative pain scores, we still advocate for conservative treatment before opting for surgery. In the discussion section we emphasized conservative treatment prior to surgery.
Changes in the text 2: See page 10-11, lines 215 and 224.

Comment 3: I like that the authors emphasize a complete evaluation and trial of conservative therapy prior to recommending this operation.
Answer 3: Thank you for your compliment.
Changes in the text 3: No changes were made.

Reviewer D

Comment 1: Baseline characteristics of the patients present some significant differences, most strikingly in gender Male Vs Female. I would appreciate if baseline characteristics (male/female, previous surgeries or not, length and angle of xiphysternum etc were also analyzed by means of t-student or chi-square as appropriate, and p values included in table 1. You may find significant differences and in that case I would include this in the discussion. Male prevalence seems very striking, can this also explain or partially explain pathophysiology?

Answer 1: First of all, thank you for your thorough review of our manuscript and the comments raised. As already addressed by Reviewer B: our number of events and total number of patients are too small to perform any meaningful statistical analysis to which we therefore deferred from. Nevertheless, we interpreted the trends observed, such as the tilted male-female ratio. For example, seventy-nine percent of patients were male, suggesting that the male sex is a predisposing factor for xiphodynia. This is in accordance with the case series by Dorn et al, in which 11 out of 11 patients were male. Conclusively, the male sex might play a role in the pathophysiology of xyphodynia. However, this is subject to further research.

We have added baseline characteristics trends to the discussion.

Changes in the text 1: See Page 9, lines 181-186.

Comment 2: Why do you use vicryl to close the midline fascia? In my experience, non-absorbable heavy monofilament appears more appropriate to avoid hernias, especially in obese patients. Please explain this choice.

Answer 2: Based on the experience in our clinic we routinely use vicryl for fascia closure because the non-absorbable sutures may provide more irritation to the thin substernal skin. Furthermore, the incisions are small, and it takes more than 6 weeks for vicryl to resolve, based on which we feel that this should be sufficient for adequate wound healing. This was added to the discussion section.

Despite we observed no incisional hernias, we agree that nonabsorbable sutures are associated with a reduced frequency of hernias based on a meta-analysis by Hodgson NC, Malthaner RA, Ostbye T. The search for an ideal method of abdominal fascial closure: a meta-analysis. Ann Surg. 2000;231(3):436-442. Nevertheless, they also described reduced pain and wound sinuses using absorbable versus nonabsorbable

sutures, which were favored by us.
Changes in the text 2: See Page 6, lines 104-105.

Comment 3: You state that 3 out of 5 patients who were not symptoms-free after surgery had received previously analgesia with good response. Why were they then selected for surgery? Please clarify this point
Answer 3: Thank you for your comment. All patients selected for surgery did not have sufficient response (read: only partial) to previously analgesia. Patients who were pain free after analgesia were not treated by xiphoidectomy. However, we agree that our writing may have not been sufficient and have amended the manuscript
Changes in the text 3: See page 8, line 171.