

COMMENTS TO AUTHORS 1

The first sentence of the Results has already been written in Materials and Methods. The Reference 24 is the same of the 9. In the pN staging of the table 1 the Authors put also the N4, but it doesn't exist. Furthermore in the table 1 in the TEA column the numbers of the pT and pN are the same: is it a typing mistake?

Answer:

- Q:** "In the pN staging of the table 1 the Authors put also the N4, but it doesn't exist"
A: pN staging of the table 1 was a mistake, change: N0/1/2/3/4 to N0/1/2/3a/3b.
- Q:** "In the table 1 in the TEA column the numbers of the pT and pN are the same: is it a typing mistake?"
A: No, they are just same coincidentally.

COMMENTS TO AUTHORS 2

Hereby I would like to comment on the article entitled: "Transcutaneous electroacupuncture alleviates postoperative ileus after gastrectomy; A randomized clinical trial" by the authors Chen KB et al. The authors show in a randomized clinical trial that transcutaneous electroacupuncture reduces time to first flatus or defaecation following gastrectomy. This is an interesting concept and the authors should be congratulated on performing this study. However, I have some major concerns 1. The authors state that postoperative enema was an exclusion criteria. This suggests that patients were excluded after randomization. However this is not described in the methods section. Furthermore, it is unclear how patients were randomized (was it done by computer randomization) and it was unclear whether patients in the control group also were given the wrist band. 2. Regarding POI; there is no clear definition in the materials and methods, I would suggest using the definition by Vather et al (J Gastrointest Surg. 2013 May; 17(5):962-72). It is unclear how time to first flatus or defaecation was assessed. Did the patients receive diaries? The authors do not show a difference in POI, but a difference in time to first flatus or defaecation, this should be stated more clear. The title is misleading. 3. How did the authors assess bowel sounds? In my view this is a very unreliable parameter and it would be better to omit these results. 4. The type of gastrectomy (total or partial) needs to be included in the baseline characteristics. 5. The nasogastric tube duration is long and does not reflect current clinical practice. Why was this

done? Did the patients get postoperative ERAS treatment? Minor 1. The discussion needs to be compacted 2. The reason for withdrawal of the 3 patients should be stated when known

Answer

1. **Q:** "The authors state that postoperative enema was an exclusion criteria. This suggests that patients were excluded after randomization. However this is not described in the methods section."

A: No patient received postoperative enema, so we did not mention it in the manuscript.

Q: "it is unclear how patients were randomized (was it done by computer randomization) and it was unclear whether patients in the control group also were given the wrist band."

A: The 63 patients were randomly allocated by computer algorithm to the TEA ($n = 33$) or control ($n = 30$) group. To make the sentence clearer: The control group received no TEA treatment and without any TEA being banded.

2. **Q:** "Regarding POI; there is no clear definition in the materials and methods, I would suggest using the definition by Vather et al (J Gastrointest Surg. 2013 May; 17(5): 962-72). It is unclear how time to first flatus or defaecation was assessed. Did the patients receive diaries? The authors do not show a difference in POI, but a difference in time to first flatus or defaecation, this should be stated more clear. The title is misleading."

A: No patients received diaries. There might be some misunderstandings between postoperative ileus (POI) and prolonged postoperative ileus. As far as we concerned, POI is a status of bowel function recovering for all postoperative patients; while prolonged POI is defined as not passing flatus at > 5 d after surgery, which was mentioned in the MATERIALS AND METHODS.

3. **Q:** "How did the authors assess bowel sounds? In my view this is a very unreliable parameter and it would be better to omit these results."

A: We assessed bowel sounds by auscultation. Basically, experienced doctors are able to record bowel sounds, moreover, we spent five or more minutes to auscultate bowel movements each time, and calculated bowel sound per minute on average.

4. **Q:** "The type of gastrectomy (total or partial) needs to be included in the baseline characteristics."

A: We divided gastrectomy type by Billroth-I/II/Rou-en-Y. Among them, Billroth-I/II are partial gastrectomy, while Rou-en-Y represents total gastrectomy.

5. **Q:** "The nasogastric tube duration is long and does not reflect current clinical practice. Why was this done? Did the patients get postoperative ERAS treatment?"

A: Nasogastric tube duration is important outcome, because we normally remove nasogastric tube after first flatus in our center, which more or less reflects the bowel motility recovery, although it is recommended to remove it at the POD 1 or 2 according to ERAS. **What's more, if the volume of nasogastric tube drainage is over 100 ml, we will postpone the time of tube removal.**

6. **Q:** "The reason for withdrawal of the 3 patients should be stated when known"

A: The 3 patients withdrew the research without any reason.

COMMENTS TO AUTHORS 3

This study investigated an innovative intervention for POI and found that TEA may help with the recovery of POI. The results are interesting and promising, especially with such a small and portable device. Nevertheless, the study design consists of many biases that may influence the objectiveness of the study. Some are listed as follows: 1. One major issue of POI study is that most outcome parameters are subjective. Therefore, keeping doctors and patients blinded from the study is the priority to ensure the objectiveness of the results. In this study, it was highly possible to fulfill that since the device is very small and there was little difference between on and off. Giving control patients sham stimulation should be conducted for this reason. However, as far as I see, no such sham group was included. Both patients and doctors or even researchers are aware of the grouping, this causes inevitable biases. 2. The authors should explain how were the patients divided into different groups, by envelope, random number, or program? This is also very important issue that should be very carefully handled in such study design. 3. The primary outcome should be ONE single outcome that is closely related to the research outcome. Instead, the authors chose three. Please explain carefully why three and why these three? 4. According to the literature, the most reliable parameter for POI is passage of feces and tolerance of solid food intake (published on Annals of Surgery by an Amsterdam group). Neither of them was included in the analysis. Instead, the authors chose passage of flatus and bowel movement, both are considered as not reliable by many studies. 5. In the methods, the authors should explain the definition or how each parameter was measured.

Answer:

1. **Q:** "One major issue of POI study is that most outcome parameters are subjective. Therefore, keeping doctors and patients blinded from the study is the priority to ensure the objectiveness of the results."

A: Actually, time to first flatus is objective as well as the postoperative symptom assessment by patients themselves. And the tolerance to liquid and semi-liquid was also assessed in our research. Another aspect: Some studies have inserted a Sitz marker capsule into the distal anastomosis to evaluate bowel movements. However, it is invasive and not accepted by all surgeons. Taken together, there should be better assessments for postoperative bowel motility, maybe they will be developed in the future with less risky and non-invasive techniques.

Q: "In this study, it was highly possible to fulfill.... However, as far as I see, no such sham group was included."

A: It was not a double-blind research, thus we could not exclude the placebo effect.

2. **Q:** "The primary outcome should be ONE single outcome that is closely related to the research outcome. Instead, the authors chose three. Please explain carefully why three and why these three?"

A: In our research, the main outcomes were hours to the first flatus or bowel movement, time to nasogastric tube removal, time to liquid and semi-liquid diet, and hospital stay. The reasons why we chose these outcomes was that all of them directly or indirectly reflected the bowel motility after gastrectomy, and not single outcome could make the conclusion more convincing.

3. **Q:** "The authors should explain how were the patients divided into different groups, by envelope, random number, or program?"

A: The 63 patients were randomly allocated by computer algorithm to the TEA ($n = 33$) or control ($n = 30$) group.

4. **Q:** "According to the literature, the most reliable parameter for POI is passage of feces and tolerance of solid food intake (published on Annals of Surgery by an Amsterdam group). Neither of them was included in the analysis...."

A: Clinically, the first flatus, tolerance of liquid or semi-liquid diet are more available, because we discharged the patients as long as they could tolerate semi-liquid with first flatus. Passage of feces are influenced by more factors such as bowel habits.

5. **Q:** "In the methods, the authors should explain the definition or how each parameter was measured"

A: The secondary outcomes included postoperative symptom assessment based on each patient's subjective scale (0-10, including pain, tiredness, nausea, shortness of breath, and wellbeing according to Edmonton Symptom Assessment System) and postoperative complications. Prolonged POI was defined as not passing flatus at > 5 d after surgery, which was mentioned in MATERIALS AND METHODS. All other parameters are common to clinical doctors.

COMMENTS TO REVISED MANUSCRIPT

Q: The authors have addressed my questions adequately in most cases. I would suggest adding the reason for not removing the nasogastric tube as state by Dr Chen in his reply in the text and address the lack of a standardized ERAS protocol. Furthermore, I would omit data on bowel sounds.

A: I have added one more sentence in the MATERIALS AND METHODS section according to the comments to revision—" As it is not yet started of enhanced recovery after surgery (ERAS) protocol in this study, we routinely removed nasogastric tube after first flatus in our center unless the volume of nasogastric tube drainage is over 100 ml per day". Besides, on my standpoint, I would like not to omit datas of bowel sounds, since we noticed the phenomenon that quantity or quality of bowel sounds was a good predictor of bowel motility recovery in case that we listened to bowel sounds of each patient very seriously for 5 or more minutes to make the datas credible, additionally, bowel sounds measurement is still used in most related publications (<http://aim.bmj.com/content/32/3/223.long>; <https://onf.ons.org/onf/40/2/beneficial-effect-st-36-zusanli-acupressure-postoperative-gastrointestinal-function>)as well as other publications in chinese (<http://www.cqvip.com/QK/96076X/201603/669239456.html>).