Dear Editor and Reviewers,

Re: Response to the manuscript "*Effects of transcutaneous electrical acupoint stimulation (TEAS) on colonic spasm during colonoscopy: A randomized controlled trial*" (Manuscript NO.: 123453)

We greatly appreciate the time and effort invested by the editors and reviewers in the manuscript. Based on these comments and suggestions, we have made careful modification on the original manuscript. A manuscript with only the revised or newly added content highlighted in yellow has been uploaded as the main manuscript. The reviewers' questions were answered below. Once again, we acknowledge the comments and constructive suggestions very much, which are valuable in improving the quality of our manuscript. Here are our responses to the reviewers' comments point-by-point.

Response to Reviewer #1:

Specific Comments to Authors

Q1. The serious lack of intervention details is the biggest flaw in this manuscript. The repeatability of TEAS depends entirely on the stimulation parameters. The article only mentioned 'current intensity gradually increased to the maximum level that patients can tolerate', but did not report the average/median current intensity (mA) actually used by each patient or group of patients. This makes it impossible for any other researcher to accurately repeat your experiment. The specific value of the current intensity (mean ± standard deviation or median and range) must be supplemented.

Response:

We sincerely thank you for this valuable and thoughtful comment. In response, we have carefully reviewed the original intervention records for PC6, TE6, ST36, and ST37 in the 71 patients in Group T and added the

corresponding output intensity data to the revised manuscript. The device output was adjustable on a scale of 0-10, with a maximum root mean square output current of no more than 10 mA across a 250-Ω resistive load. These data are presented as the median (Q1, Q3). The median output intensity settings were 7 (7, 8) at PC6, 8 (7, 8) at TE6, 8 (7, 9) at ST36, and 9 (9, 10) at ST37.

We appreciate your suggestion, which has helps us provide a clearer and more complete description of the TEAS intervention.

Corrections in the revised manuscript: Page 7, Line 14 ,18 and 22-25; Page 10, Line 23-25.

Q2. The authors defined spasm as 'lumen patency less than two-thirds of the maximum diameter'. How is the threshold determined? Is there any reference support? How to measure the proportion of 'two-thirds' objectively and in real time in dynamic endoscopic operation? This is easily affected by the subjective judgment of endoscopists. It is recommended that the authors provide more objective criteria such as the use of a validated spasticity scale, or blind evaluation by a third party through video, or at least a detailed description of the judgment process to reduce bias.

Response:

We sincerely thank you for this important and constructive comment. Accordingly, we have revised the manuscript and would like to provide the following clarification in response to your questions.

The assessment method used in our study was based on previous studies *(Nemoto D et al, 2019; Chen et al, 2022; Wang et al, 2025)*. In these studies, colonic spasm was evaluated in real time according to the degree of luminal opening during colonoscopy. Spasm was identified when the luminal opening of an observed colonic segment narrowed to less than two-thirds of its maximum diameter when fully dilated. We applied the same method throughout colonoscope insertion and withdrawal and have described the

judgment process more clearly in the Methods section. In addition, to improve the consistency of the assessment and reduce observer bias, the primary outcome was jointly evaluated by two experienced endoscopists who had received standardized training before the study. Colonic spasm was recorded only when both endoscopists agreed. However, because no universally accepted guideline or validated scale is currently available for the objective assessment of colonic spasm, some degree of subjectivity remains unavoidable. We have acknowledged this issue in the Limitations section and noted that further studies are needed to validate and standardize the assessment method.

Thank you again for your insightful comments.

Reference:

1. Nemoto D, Suzuki S, Mori H, et al. Inhibitory effect of lidocaine on colonic spasm during colonoscopy: A multicenter double-blind, randomized controlled trial. *Dig Endosc*. 2019;31(2):173-179. [DOI: 10.1111/den.13272]
2. Chen B, Ma Y, Zhong C, et al. Suppression effect and safety of acupuncture on colonic spasm during colonoscopy: A randomized controlled trial. *J Gastrointest Oncol*. 2022;13(3):1169-1177. [DOI: 10.21037/jgo-22-407]
3. Wang Q, Sui Y, Gong J, Chen X. Operation sequence of bidirectional endoscopy with topical anesthesia affected colonoscopy performance: A randomized controlled trial. *BMC Gastroenterol*. 2025;25(1):121. [DOI: 10.1186/s12876-025-03721-5]

Corrections in the revised manuscript: Page 9, Lines 2-7; Page 15, Line 23-26.

Q3. The method mentioned that ' any spasm that lasts more than 180 seconds will be terminated with scopolamine and such cases will be excluded from the analysis of the protocol '. This may lead to an imbalance in the characteristics of excluded cases between the two groups. For example, the TEAS group had mild spasm, which was not likely to exceed 180 seconds ; the control group

had severe spasm, which was more likely to exceed 180 seconds and be excluded, thus artificially ' purified ' the control group data. Intentionality analysis (ITT) is recommended to include all randomized patients in the primary outcome analysis, including those who used remedial drugs (the outcome can be recorded as ' treatment failure ' or ' spasm ').

Response:

We sincerely appreciate your careful review and helpful comment. We agree that excluding patients who experienced colonic spasm lasting more than 180 seconds was inappropriate and could introduce bias into the primary outcome analysis. Accordingly, we have revised the exclusion criteria and reanalyzed all available data under the revised criteria.

After carefully reviewing the study records, we confirmed that no participant in either group experienced colonic spasm lasting more than 180 seconds or received rescue scopolamine. Therefore, no participant was excluded from the analysis for this reason, and the reported primary outcome results were unaffected. The Methods section now only describes the rescue treatment for colonic spasm persisting for more than 180 seconds as a safety measure and the Results section states that no such event occurred in either group. We apologize for this inappropriate methodological description in the original manuscript. We sincerely appreciate your valuable comment, which helps us identify and correct this issue and improve the methodological clarity of the manuscript.

Corrections in the revised manuscript: Page 9, Line 10-13; Page 11, Line 14-16.

Q4. Table 3 Data interpretation error : The median of ' postoperative pain ' was 2 (0, 3) in the TEAS group and 1 (0, 2) in the control group, and P = 0.019. This indicates that the pain score of the TEAS group is higher ! This is completely contradictory to the full text description ' Group T demonstrated reduced postoperative pain '.

Response:

Thank you for carefully identifying this inconsistency. We sincerely apologize for this oversight. The correct median postoperative pain score was 1 (0, 2) in Group T and 2 (0, 3) in Group C (P = 0.019). The statistical analysis was performed using the accurate group data, and it did not affect the statistical results or the conclusions of the study. We have corrected Table 3 and the corresponding numerical description in the Results section. We also carefully rechecked the remaining data in the manuscript and confirmed their accuracy.

Thank you again for your helpful suggestions.

Corrections in the revised manuscript: Page 11, Line 24; Page 30, Table 3.

Q5. The discussion section is slightly far-fetched about the explanation of ' why TEAS shortens the duration of spasm (main outcome) but fails to significantly shorten the colonoscopy insertion time ' (' related to the effect of limited TEAS time '). It is suggested to further explore whether the preventive effect of TEAS is systemic and limited, and the preoperative single stimulation is not enough to completely cover the whole operation process, because the colonoscopy operation itself (gas injection, traction) will continue to induce new spasms ? This provides a good hypothesis for future research such as intraoperative continuous TEAS.

Response:

Thank you very much for this insightful and constructive comment. We apologize that our original explanation of this finding was not sufficiently clear. We have revised the Discussion to clarify that a single preoperative TEAS session may have a limited duration, and continued endoscopic manipulation may further reduce its effect during the procedure. A recent randomized trial has shown that TEAS applied continuously during surgery reduced emergence agitation, supporting the potential value of sustained intraoperative stimulation. Accordingly, we also have revised the Limitations

to indicate that future studies should investigate whether continuous intraoperative TEAS can provide more sustained control of colonic spasm

We sincerely appreciate your valuable suggestion, which helps us improve the interpretation of our findings.

Reference:

Li X, Wu T, Li W, Xing Q, Bao W. Preventive effect of transcutaneous acupoint electrical stimulation on emergence agitation after general anesthesia in pediatric patients undergoing tonsillectomy: A randomized clinical trial. Medicine (Baltimore). 2026;105(7):e47737.

Corrections in the revised manuscript: Page 14, Line 10-17; Page 15, Line 28-29; Page 16, Line 1-2.

Response to Reviewer #2:

Specific Comments to Authors

Colonic spasm frequently occurs during painless colonoscopy, directly hindering endoscopic procedures and impairing patient recovery. Pharmacological antispasmodics are commonly used but have limited efficacy. In this study, Chen et al. investigated the effect of preoperative transcutaneous electrical acupoint stimulation (TEAS) on the incidence of colonic spasm during painless colonoscopy.

They analyzed a total of 146 outpatients who underwent painless colonoscopy. The patients were randomly assigned to either the TEAS group (Group T) or the control group (Group C). Group T was subject to TEAS for 20 minutes before anesthesia induction, while Group C received electrode attachment but without stimulation. As a result, Group T demonstrated a significantly lower incidence of colonic spasm, a shorter spasm duration, reduced postoperative pain, lower incidences of some adverse events. They concluded that TEAS is an effective, safe, non-pharmacological option for painless colonoscopy.

This is a challenging study that combines Chinese traditional medicine

and Western medicine for regulating colonic spasm during colonoscopy. Since TEAS is non-invasive and probably will be at low cost, it will be a promising option for controlling colonic spasm. This might be particularly acceptable for people who live in east Asia.

As such, this is a potentially attractive study, but I have some concerns in this study design.

Response:

We sincerely thank you for your thoughtful and encouraging comments. We are pleased that you recognize the clinical significance of our study. Your positive evaluation is highly motivating and reinforces the relevance of our work, and we will continue to refine and expand upon it in future research.

Major points:

Q1. In the exclusion criteria, the authors document that patients who developed hemodynamic instability during TEAS or required supplemental antispasmodics during colonoscopy were excluded from the final analysis. However, the former is a kind of perioperative outcome similar to hypotension and requirement for norepinephrine and the latter suggests spasm duration of longer than 180 seconds (Table 1 and pages 8, line 28). It may not be appropriate to exclude these patients. After modifying parameters of perioperative outcomes, these excluded cases should be included and re-analyzed.

Response:

We sincerely thank you for this important methodological comment. We apologize for the inappropriate descriptions in the original manuscript and agree that the patients identified in your comment should not have been excluded from the analysis. Accordingly, we have revised the exclusion criteria and reanalyzed all available data under the revised criteria.

After carefully reviewing the study records, we confirmed that no participant in either group developed hemodynamic instability during the assigned intervention. In addition, no participant experienced colonic spasm

lasting more than 180 seconds or required rescue scopolamine. Therefore, no participant was excluded for either reason, and the reported results remained unchanged. The Methods section has been revised to clarify that, if hemodynamic instability occurred during the assigned intervention, stimulation would be discontinued, appropriate clinical management would be provided, and the event would be recorded as an adverse event rather than used as a reason for exclusion. For colonic spasm persisting for more than 180 seconds, rescue scopolamine has been retained as a safety measure, while the statement excluding such patients from the analysis has been deleted. The Results section now states that neither of these events occurred in the present study.

We sincerely appreciate your careful review and helpful suggestion, which improves the methodological clarity of the manuscript.

Corrections in the revised manuscript: Page 7, Line 28; Page 8, Line 1-3; Page 9, Line 10-13; Page 11, Line 14-16; Page 12 Line 5-7.

Q2. It is not clear how colonic spasm was detected. Although the authors claim that colonic spasm was defined as lumen patency less than two-thirds of the maximum diameter, how did they identify colonic spasm that may occur anywhere throughout the entire colon during colonoscopy? For example, can you detect colonic spasm in the ascending colon while you are observing sigmoid colon?

Response:

We sincerely thank you for this insightful comment and would like to provide the following clarification in response to your questions.

In our study, the primary outcome was assessed using the endoscopic method reported in previous studies *(Nemoto et al, 2019; Chen et al, 2022; Wang et al, 2025)*. During colonoscope insertion and withdrawal, each colonic segment was assessed sequentially when it was visualized on the endoscopic monitor. Two experienced endoscopists who had received standardized

training jointly evaluated the luminal opening, and spasm was recorded only when both agreed that the luminal opening of any observed segment had narrowed to less than two-thirds of its maximum diameter when fully dilated. However, because no universally accepted criterion is currently available for assessing colonic spasm during colonoscopy, we acknowledge that this method has inherent limitations. In particular, it cannot assess the entire colon simultaneously and may therefore miss transient spasms in unobserved segments. Therefore, while the sigmoid colon is being observed, a transient spasm occurring in the ascending colon cannot be detected at that time. We have added this inherent limitation to the revised manuscript and described the assessment process more clearly in the Methods section.

Thank you again for your valuable comment.

Reference:

1. Nemoto D, Suzuki S, Mori H, et al. Inhibitory effect of lidocaine on colonic spasm during colonoscopy: A multicenter double-blind, randomized controlled trial. *Dig Endosc*. 2019;31(2):173-179. [DOI: 10.1111/den.13272]
2. Chen B, Ma Y, Zhong C, et al. Suppression effect and safety of acupuncture on colonic spasm during colonoscopy: A randomized controlled trial. *J Gastrointest Oncol*. 2022;13(3):1169-1177. [DOI: 10.21037/jgo-22-407]
3. Wang Q, Sui Y, Gong J, Chen X. Operation sequence of bidirectional endoscopy with topical anesthesia affected colonoscopy performance: A randomized controlled trial. *BMC Gastroenterol*. 2025;25(1):121. [DOI: 10.1186/s12876-025-03721-5]

Corrections in the revised manuscript: Page 9, Line 2-7; Page 15, Line 23-28.

Minor points:

Q1. A concise figure illustrating four acupoints in the body will be useful for easy understanding by the audience.

Response:

We sincerely thank you for this constructive suggestion. According to your insightful comment, we have added a new figure showing the bilateral locations of Neiguan (PC6), Zhigou (TE6), Zusanli (ST36), and Shangjuxu (ST37), together with enlarged views of each acupoint. We greatly appreciate this valuable suggestion, which has made the description of the TEAS intervention clearer and easier to understand.

Corrections in the revised manuscript: Page 7, Line 18-19 and 24-25; Page 25, Figure 1.

Q2. In page 6, line 1, what does "qi" mean? (in the context that , … regulating qi, harmonizing the stomach, and …)

Response:

Thank you very much for pointing out this unclear terminology. "Qi" is a concept used in traditional Chinese medicine to describe the functional activity or vital energy of the body. We agree that this term may be unfamiliar to international readers. Therefore, we have removed this terminology and revised the sentence using a clearer description of the traditionally recognized gastrointestinal and calming effects of these acupoints.

Corrections in the revised manuscript: Page 5, Line 27-30.

Q3. In page 9, line 7, PDR should be spelled out.

Response:

Thank you very much for this careful comment. We apologize for any confusion regarding the abbreviation. After rechecking the manuscript, we confirmed that "PDR" was defined as "polyp detection rate (PDR)" at its first occurrence in the Introduction (page 5, line 4). We have retained the abbreviation throughout the remainder of the manuscript to maintain consistency. In addition, we carefully rechecked the abbreviations throughout the manuscript to ensure that they were appropriately defined and used consistently.

Corrections in the revised manuscript: Page 9, Lines 18-19.

Q4. In page 14, lines 10-12, the meaning of the sentence "Studies indicate, …, corresponding to the postoperative …" cannot be understood.

Response:

Thank you very much for pointing out this unclear statement. We apologize for the inaccurate wording in the original manuscript. We have revised the relevant sentence as follows: "Previous studies have shown that preoperative TEAS has analgesic effects, which is consistent with the lower postoperative pain scores observed in Group T in the present study."

We sincerely appreciate this helpful comment, which allows us to improve the presentation of this part of the Discussion.

Corrections in the revised manuscript: Page 14, Line 22-24.

Q5. Please explain the objective criterion for identifying colonic spasm based on the previously established "scoring system" (page 15, lines 9-11). This content may not be documented in the Method. Also, please show the reference.

Response:

We appreciate your careful attention to the assessment of colonic spasm. We apologize for the incorrectly referring to the assessment method as a "previously established scoring system." There is currently no established scoring system for assessing colonic spasm during colonoscopy. The assessment in our study was based on the method described by *Nemoto et al.*, in which colonic spasm was identified when the luminal opening narrowed to less than two-thirds of its maximum diameter when fully dilated. We have corrected this error and revised the relevant description accordingly. We sincerely thank you for this helpful comment.

The supporting reference is as follows:

Nemoto D, Suzuki S, Mori H, et al. Inhibitory effect of lidocaine on colonic

spasm during colonoscopy: A multicenter double-blind, randomized controlled trial. Dig Endosc. 2019;31(2):173 – 179. [DOI:10.1111/den.13272].

Corrections in the revised manuscript: Page 15, Line 23-26 and 28-29; Page 16, Line 1-2.

Q6. In Table 2, does colonoscope insertion time mean PDR? If so, please unify them.

Response:

We sincerely thank you for pointing out this potential ambiguity. Colonoscope insertion time and PDR are two distinct outcomes. PDR refers to the polyp detection rate, and colonoscope insertion time refers to the interval from the initial insertion of the colonoscope until the ileocecal valve is reached. To avoid further confusion, we have revised the relevant description in the Methods section for greater clarity. We have also standardized the presentation of abbreviations in Table 2 and provided their full terms in the table footnote.

Thank you again for this helpful suggestion.

Corrections in the revised manuscript: Page 9, Line 18-21; Page 29, Line 4-5.

Response to Reviewer #3:

Specific Comments to Authors

This manuscript presents a prospective randomized controlled trial investigating the efficacy of transcutaneous electrical acupoint stimulation (TEAS) in preventing colonic spasm during painless colonoscopy. The manuscript is generally well organized, the methodology is clearly described, and the findings are of potential clinical interest. I recommend Minor Revision before publication.

Response:

We sincerely thank you for your thoughtful and encouraging evaluation of our manuscript. We sincerely thank you for your positive appraisal and

generous recognition of our work. Your positive feedback on the clarity of our writing is greatly appreciated. We are grateful for your time and constructive comments, which reinforce our confidence in the significance and contribution of our work.

Major Comments

Q1. Clarification of blinding procedures: The authors state that outcome assessors were blinded to group allocation; however, additional details regarding the blinding process would strengthen the methodological rigor of the study. Since patients in the TEAS group experienced electrical stimulation whereas those in the control group did not, it is possible that participants were aware of their treatment assignment. The authors are encouraged to further describe the blinding procedures and briefly discuss the potential influence of placebo effects, particularly on subjective outcomes such as pain and satisfaction scores.

Response:

We appreciate your careful consideration of the blinding procedures. The blinding procedures implemented in this study were consistent with the registered blinding plan. Participants were not informed of their group allocation, and identical electrode pads were applied at the same acupoints in both groups. The investigators responsible for endoscopic and postoperative outcome assessment were not involved in allocation or intervention delivery and remained blinded to group assignment.

We agree that the perceptible sensations produced by TEAS may have enabled some participants to infer their allocation, though no current was delivered in the control group. Therefore, complete participant blinding could not be fully ensured. We have added further details of the blinding procedures to the Methods and briefly discussed the possible influence of placebo effects on patient-reported outcomes, particularly postoperative pain and patient satisfaction. We appreciate this suggestion, which has helps us describe the study procedures and interpret the subjective outcomes more

appropriately.

Corrections in the revised manuscript: Page 7, Line 12-13; Page 8, Line 3-5; Page 9, Line 4-7.

Q2. Assessment of the primary outcome: The definition of colonic spasm was based on a previously reported criterion; however, assessment of colonic spasm may still involve a degree of subjective judgment. The authors should provide additional information regarding how the outcome was evaluated, including whether endoscopists received standardized training and how potential discrepancies in assessment were addressed. This would improve confidence in the reliability of the primary endpoint.

Response:

We sincerely thank you for this valuable comment. In our study, colonic spasm was assessed jointly by two experienced endoscopists, both of whom had received standardized training in the assessment of the primary outcome before study initiation. Throughout colonoscope insertion and withdrawal, they assessed each colonic segment visualized on the endoscopic monitor using the predefined two-thirds threshold. Any discrepancy was resolved through immediate discussion, and spasm was recorded only when both endoscopists reached agreement. We have clarified these details in the Methods section and acknowledged the remaining subjectivity of this assessment in the Limitations. We sincerely appreciate your helpful suggestion.

Corrections in the revised manuscript: Page 9, Line 2-7; Page 15, Line 23-26.

Q3. Reporting of effect sizes: The statistical analysis primarily focuses on P values. To enhance the clinical interpretability of the findings, the authors are encouraged to report effect size measures for the primary outcome, such as relative risk (RR), along with corresponding 95% confidence intervals.

Response:

We sincerely thank you for this valuable suggestion. We therefore calculated the relative risk and corresponding 95% confidence interval for the incidence of colonic spasm. The RR was 0.41 (95%CI: 0.18–0.93). This information has been added to the Statistical analysis section and the reporting of the primary outcome in both the Abstract and Results.

Thank you again for this helpful suggestion.

Corrections in the revised manuscript: Page 3, Line 25; Page 10, Line 11-12; Page 11, Line 4.

Q4. Further discussion of study limitations: The limitations section is appropriate but could be slightly expanded. In particular, the authors may wish to acknowledge that the sham procedure may not have completely eliminated potential placebo effects and that the study population was limited to relatively low-risk patients (ASA I–II), which may affect the generalizability of the findings to broader patient populations.

Response:

We appreciate your positive assessment of the limitations section and thank you for these constructive suggestions for further improvement. We agree that both the restricted study population and the sham procedure should be acknowledged as potential limitations.

In response, we have expanded the Limitations to address both concerns. The revised text now clarifies the potential limitation in generalizability and the possible influence of placebo effects on subjective outcomes, allowing for a more cautious and balanced interpretation of our findings. We appreciate this helpful comment, which improves the completeness of the limitation discussion.

Corrections in the revised manuscript: Page 14, Line 25-27; Page 15, Line 16-19 and 27-28.

Minor Comments

Q1. Clinical trial registration information: The manuscript states that the trial was registered at ClinicalTrials.gov, whereas the registration number provided (ChiCTR2500114656) appears to correspond to the Chinese Clinical Trial Registry (ChiCTR). The authors should verify and correct the registration platform information if necessary.

Response:

We sincerely thank you for carefully identifying this inconsistency. After verifying the registration record, we confirmed that the trial was registered with the Chinese Clinical Trial Registry (ChiCTR), rather than ClinicalTrials.gov. We have corrected the registry name and have rechecked the related information throughout the manuscript to ensure accuracy. We appreciate this helpful comment.

Corrections in the revised manuscript: Page 6, Line 17-18; Page 23, Line 17-18.

Q2. Allocation concealment: Although randomization was performed using a random number table, the method used for allocation concealment is not clearly described. A brief explanation of how treatment assignments were concealed before enrollment would improve adherence to CONSORT reporting standards.

Response:

We sincerely thank you for this important comment. In this study, group assignments were concealed in opaque, sealed envelopes, which were assigned according to the order of enrollment and opened only immediately before the assigned intervention. Thus, treatment assignments remained concealed during enrollment and until intervention delivery. We have added this information to the revised manuscript to provide a clearer description of the randomization procedure and improve adherence to the CONSORT reporting standards.

Thank you again for this helpful suggestion.

Corrections in the revised manuscript: Page 7, Line 10-12; Page 8, Line 3-5.

Q3. Language and wording: The overall language quality of the manuscript is satisfactory. Nevertheless, a few sentences could be further optimized for clarity and grammatical accuracy. For instance, "has been shown to benefit in gastrointestinal regulation" may be revised to "has been shown to be beneficial for gastrointestinal regulation," and "objective criterion for identifying colonic spasm" may be revised to "an objective criterion for identifying colonic spasm." A careful check for minor grammatical inconsistencies throughout the manuscript is recommended.

Response:

We sincerely thank you for your positive assessment of the overall language quality of our manuscript and for pointing out these wording issues. Following your suggestions, we have made the corresponding revisions and carefully reviewed the entire manuscript, with particular attention to grammatical accuracy, clarity of expression, and consistency of terminology and wording across different sections. We have also thoroughly polished the language throughout the manuscript. These suggestions were particularly helpful in refining the manuscript.

Corrections in the revised manuscript: Page 3, Line 7; Page 5, Line 2; Page 13, Line 1.

The study is clinically relevant and contributes valuable evidence regarding the use of TEAS during painless colonoscopy. Addressing the comments above would further strengthen the manuscript and improve its clarity and methodological transparency.

We tried our best to improve the manuscript and made some changes in the manuscript. These revisions will not influence the content and framework of the paper. And here we did not list the changes but highlighted in yellow in revised manuscript. We sincerely appreciate the time and effort that the

editors and reviewers have invested in evaluating our manuscript. We are eager to receive any additional feedback or suggestions that may further enhance our work.

Round 2

Dear Editor,

Thank you for giving us the opportunity to further revise our manuscript entitled "*Effects of transcutaneous electrical acupoint stimulation (TEAS) on colonic spasm during colonoscopy: A randomized controlled trial*" *(Manuscript No. 123453)*. We sincerely appreciate your careful guidance and the reviewer's helpful comments. We have addressed each point and revised the manuscript accordingly. Our point-by-point responses are provided below. We sincerely hope that these responses can alleviate your concerns, and look forward to hearing from you.

Sincerely,

Na Xing
MD, PhD, Department of Anesthesiology, Pain and Perioperative Medicine
The First Affiliated Hospital of Zhengzhou University
No.1 East Jianshe Road, Zhengzhou, Henan, China
Email: fccxingn@zzu.edu.cn

Q1. In the "Results-Adverse Events" section of the revised manuscript, the authors stated that "No patients in the TEAS group reported discomfort during or after the process of electrical stimulation", but did not mention whether patients in the control group experienced discomfort. Considering that patients in the control group also had electrode patches applied, although there was no electrical output, the application itself might cause discomfort in some patients, such as skin allergies or pain during the removal of adhesive tape. It is recommended to supplement similar information about the control group to make the description more comprehensive.

Response:

Thank you for this helpful comment and for pointing out this incomplete

description. After reviewing the intervention records, we confirmed that no patients in the control group reported discomfort related to electrode application or removal, and no adverse reactions associated with the sham intervention were observed. We have added the corresponding findings to the "Results-Adverse events" section to provide a more complete description of the safety outcomes in both groups.

Corrections in the revised manuscript_R2: Page 11, Line 16-18.

Q2. The supplemented data on current intensity (median) in the revised manuscript is very helpful. However, there are differences in tolerance intensity among different acupoints (e.g., ST37 is 9, PC6 is 7). If the authors could provide one or two sentences to explain whether this is related to the local nerve distribution of the acupoints, subcutaneous tissue thickness, or the general sensation of patients, it would help readers better understand these parameters.

Response:

Thank you for your recognition of the supplemented current intensity data and for this thoughtful suggestion. Previous studies have suggested that local sensory nerve distribution varies across anatomical sites and may influence tolerance to electrical stimulation [1,2]. In our study, the stimulation intensity at each acupoint was adjusted separately according to the maximum level comfortably tolerated by each patient. Accordingly, the slight differences in median tolerated intensity among the four acupoints may be related to variations in local sensory nerve distribution. We have added a brief explanation to the Discussion to help readers better understand these parameters.

Thank you again for this insightful comment.

References:

1. Dölker EM, Lau S, Bernhard MA, Haueisen J. Perception thresholds and qualitative perceptions for electrocutaneous stimulation. Sci Rep.

2022;12:7335. DOI: 10.1038/s41598-022-10708-9.
2. Wie HS, Kim SN. Therapeutic components of acupuncture stimulation based on characteristics of sensory nerve and nervous signaling pathway. J Integr Med. 2025;23(2):106–112. DOI: 10.1016/j.joim.2025.02.002.

Corrections in the revised manuscript_R2: Page 12, Line 10-13.

Q3. The statement "No patients were lost to follow-up" in the manuscript is an advantage of the RCT. It is recommended to more clearly specify here that all patients who completed the intervention (142 cases) completed the follow-up in the PACU stage, so as to ensure that readers can clearly understand the completeness of the data.

Response:

Thank you for your positive assessment and helpful suggestion. Accordingly, we have clarified the number of patients who completed the intervention and specified that all 142 patients completed follow-up in the PACU stage. The corresponding description in the "Results–Patient characteristics" section has been revised to present the completeness of the follow-up data more clearly.

Corrections in the revised manuscript_R2: Page 10, Line 7-8.

Clinical trial registration statement: Any research study (clinical trial) that prospectively assigns human participants or groups of humans to one or more health-related interventions to evaluate the effects on health outcomes must be registered. Authors have 6 mo from the first patient enrollment to register the trial, but BPG recommends registration prior to enrollment. This registration policy applies to prospective, randomized, controlled trials only. Authors must provide the registration identification number and the URL for the trial's registry. In addition, the registration information must be provided in a PDF format, and the registered URL and registration identification number must also be mentioned as a footnote in the manuscript text.

Response:

Thank you for this important comment. After rechecking the registration record, we confirmed that the trial was registered with the Chinese Clinical Trial Registry on December 16, 2025, before the first participant was enrolled on December 20, 2025. We also verified that the registry name, registration number (ChiCTR2500114656), and registration URL are correctly provided in the manuscript (Page 6, Lines 9-10; Page 21, Lines 5-6). The required registration information has been prepared in PDF format and submitted accordingly. We sincerely appreciate your careful reminder and the time devoted to reviewing our manuscript.

We sincerely appreciate the time and effort devoted to evaluating our manuscript. We hope that the revisions have adequately addressed the comments and further improved the clarity and completeness of the manuscript. Thank you for your consideration of our revised submission.