



PEER-REVIEW REPORT

Name of journal: *World Journal of Clinical Cases*

Manuscript NO: 68632

Title: Outcome of the efficacy of Chinese herbal medicine for functional constipation: A systematic review and meta-analysis

Provenance and peer review: Unsolicited Manuscript; Externally peer reviewed

Peer-review model: Single blind

Reviewer’s code: 05505084

Position: Peer Reviewer

Academic degree: MD

Professional title: Staff Physician

Reviewer’s Country/Territory: United States

Author’s Country/Territory: China

Manuscript submission date: 2021-05-31

Reviewer chosen by: AI Technique

Reviewer accepted review: 2021-06-01 17:04

Reviewer performed review: 2021-06-01 18:05

Review time: 1 Hour

Scientific quality	<input type="checkbox"/> Grade A: Excellent <input checked="" type="checkbox"/> Grade B: Very good <input type="checkbox"/> Grade C: Good <input type="checkbox"/> Grade D: Fair <input type="checkbox"/> Grade E: Do not publish
Language quality	<input type="checkbox"/> Grade A: Priority publishing <input type="checkbox"/> Grade B: Minor language polishing <input checked="" type="checkbox"/> Grade C: A great deal of language polishing <input type="checkbox"/> Grade D: Rejection
Conclusion	<input type="checkbox"/> Accept (High priority) <input type="checkbox"/> Accept (General priority) <input type="checkbox"/> Minor revision <input checked="" type="checkbox"/> Major revision <input type="checkbox"/> Rejection
Re-review	<input checked="" type="checkbox"/> Yes <input type="checkbox"/> No



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Peer-reviewer statements	Peer-Review: [] Anonymous [Y] Onymous Conflicts-of-Interest: [] Yes [Y] No
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SPECIFIC COMMENTS TO AUTHORS

Thank you for allowing me to review this manuscript on Chinese herbal medicine for functional constipation. Scientifically, from what I can tell, this is well done. This is limited by poor writing/English and the fact that many of the cited studies are not in English. The major limitation here is that subgroup analyses by CHM ingredients is missing. Lumping all CHM into one intervention could be misleading. Both of these could be corrected, as well as the minor comments below, and this paper has the potential to be a great publication. In the supplementary documents, both the biostatistical review and non-native Speakers of English documents are just repeats of the PRISMA guidelines. Please upload these documents correctly. This is especially true because the use of English is suboptimal in several places in this manuscript. In the version I received, it noted this was submitted to the "World J of Clinical Cases", but the Title page notes it is submitted to "World J of Gastroenterology". It is much more suited for the latter. I suspect this is a system error and not an error on the part of the authors.

ABSTRACT: Scientifically, describes study well from what I can understand apart from the many grammar, formatting, word choice errors. In methods, typically would say it was a search "of" six databases not "over" six databases. This sentence makes no sense as worded: "Meta-analyses were performed to odds ratio (OR), mean differences (MD), and 95% CI using random-effects models." The sources of heterogeneity were not "discussed" using the mentioned methods. There are better word choices. Several missing spaces between words and parens makes results hard to read.

INTRODUCTION: Well written. Good summary of relevant literature. Appropriately brief. Reference 3 notes "direct costs". This does not necessarily mean what patient's pay.



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I would clarify. I would wonder if CHM is used by people outside of Asia. This intro is a little Asian-oriented. The study, reference 7, shows that patients in Taiwan use CHM often, but not that it is used by other patients. I would either find studies outside of Asia that support this statement or reword it to clarify that it is used often in Asia. Is there a specific hypothesis? Other PICO questions, i.e. outcomes? **METHODS:** There are several typographical errors, including missing spaces, commas, etc. PRISMA should be capitalized when spelled out Search Strategy can be in a supplement. The methods note databases were searched from year of inception of Rome Criteria; whereas, abstract implies databases were searched from their inception. I also would write out and cite what year Rome Criteria was developed and what year was used. It will be really important to clearly define what was considered CHM. Were all ages included, even children? All setting types (inpatient/outpatient)? The study selection process section skips right to extraction/data review and the study selection process is not described at all. There needs to be a correction for multiple observations (i.e. Bonferroni) mentioned since there is no primary outcome identified. There are several outcomes, which means there is no primary (i.e., singular) outcome. **RESULTS:** Again, several missing punctuation, spaces, etc. First sentence is missing a subject, i.e. "studies". "Ninety-seven studies were retrieved base on selection criteria" – based on There is a major limitations here that needs to be addressed. Lumping "CHM" all into one intervention limits the analysis here. The interventions needs to be separated by what ingredients. This would be much more helpful than knowing the outcomes compared to different controls (which is also helpful). I cannot assess nearly any of the original studies because they are in Chinese. Someone who is a native Chinese speaker needs to independently review these studies for accurate citations. Outcomes for BM need to be specified to a time period. BM per day? Per week? Subgroups for age, gender, outside China, only high quality studies, etc. would be helpful. Perhaps in asupplement. **DISCUSSION:** Studies



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and patients were not “recruited” for this review. “But all reported adverse effects didn’t need urgent treatment.” Is repeated from the results. I would put in one or the other, probably the discussion and not the results (unless there was an objective assessment of this). I would consider rewording to “were not serious”. “subgroup analysis according to ages and so on” Did I miss the aging subgroup? What were the others? “But we acknowledged that it was difficult to conduct blind successfully due to the special smell of CHM. And with a specific score criterion, it could reduce the possibility of bias.” I know what the authors are trying to say, but these sentences make no sense. Limitation of missing potentially relevant articles is missing. Also the geographic bias is another limitation. I would recommend a separate, single limitation paragraph that is easy to identify. Conclusion is appropriately guarded. REFERENCES: The references are not formatted in a consistent manner. Please correct. As above, I could not verify accuracy of most of the references due to them not being available in English. TABLES/FIGURES: All tables and figure need titles detailed enough to stand alone. That is, some mention of the focus of this SR/MA. As above, I think scientifically, this is well done. Some different subgroup analyses, most importantly by ingredient of CHM would be helpful. There is a lot of room for writing/English improvement. But, I think these could be overcome with some work on the authors’ part to make this work a great publication.



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Reviewer's code: 05352593

Position: Peer Reviewer

Academic degree: MD

Professional title: Doctor

Reviewer's Country/Territory: Colombia

Author's Country/Territory: China

Manuscript submission date: 2021-05-31

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Scientific quality	<input type="checkbox"/> Grade A: Excellent <input checked="" type="checkbox"/> Grade B: Very good <input type="checkbox"/> Grade C: Good <input type="checkbox"/> Grade D: Fair <input type="checkbox"/> Grade E: Do not publish
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Peer-reviewer statements	Peer-Review: [<input checked="" type="checkbox"/>] Anonymous [<input type="checkbox"/>] Onymous Conflicts-of-Interest: [<input type="checkbox"/>] Yes [<input checked="" type="checkbox"/>] No
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SPECIFIC COMMENTS TO AUTHORS

Thanks for inviting me to read this paper. This meta-analysis is well written. The authors describe that Chinese herbal medicine was associated with improved symptoms of functional constipation. This was evaluated by several outcomes, such as score on the Bristol Stool Scale, bowel movements, stool characteristics, recurrence rate, and efficacy rate. I congratulate the authors; this study has interesting findings with low heterogeneity (except bowel movements) that allow extrapolation of their findings to clinical settings. However, they included studies with a high risk of bias that must be evaluated by a sensibility analysis. I have some comments:

Abstract:

- **Aim:** The authors must provide an explicit statement of questions being addressed with reference to participants, interventions, comparisons, outcomes, and study design (PICOS).
- **Results:** o please, the I2 must be included in this section.

Introduction: As described above, the aim also must provide an explicit statement of questions being addressed with reference to participants, interventions, comparisons, outcomes, and study design (PICOS) in the introduction. A meta-analysis evaluated the efficacy and safety of CHM in the treatment of constipation. The authors must describe the difference that has that meta-analysis with their study, this would add value to their study.

Methods:

- The eligibility criteria must be described before the search strategy, as recommended by the PRISMA guideline.
- The authors must state that MD was used because the outcome measurements in all studies are made on the same scale.

Results:

- Could a sensibility analysis based on methodological quality perform?
- Please, the statement "Five studies compared CHM with western medicine and reported the recurrence rate (RR). The results showed the treatment for functional constipation was no sign in favor



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of CHM" must be corrected. Some references must be improved. These don't have pages number.