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PEER-REVIEW REPORT

Name of journal: *World Journal of Gastroenterology*

Manuscript NO: 82456

Title: Different types of fruit intake and colorectal cancer risk: a meta-analysis of observational studies

Provenance and peer review: Unsolicited Manuscript; Externally peer reviewed

Peer-review model: Single blind

Reviewer's code: 06475572
Position: Peer Reviewer
Academic degree: MD

Professional title: Doctor

Reviewer's Country/Territory: Italy

Author's Country/Territory: China

Manuscript submission date: 2022-12-19

Reviewer chosen by: AI Technique

Reviewer accepted review: 2022-12-20 05:34

Reviewer performed review: 2022-12-28 10:03

Review time: 8 Days and 4 Hours

Scientific quality	[ ] Grade A: Excellent [ ] Grade B: Very good [Y] Grade C: Good [ ] Grade D: Fair [ ] Grade E: Do not publish
Language quality	[ ] Grade A: Priority publishing [Y] Grade B: Minor language polishing [ ] Grade C: A great deal of language polishing [ ] Grade D: Rejection
Conclusion	[ ] Accept (High priority) [ ] Accept (General priority) [Y] Minor revision [ ] Major revision [ ] Rejection
Re-review	[ ] Yes [Y] No

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

SPECIFIC COMMENTS TO AUTHORS

Dear author, In this manuscript, the author discusses the relationship between different types of fruit consumption and colorectal cancer risk by using previously published studies and carrying out the meta-analysis using statistics. The incidence of colorectal cancer is a serious health problem in the Western world. Though this type of cancer has a survival rate of 91% when diagnosed at the localized stage, preventive measure is much more important. The author makes a good effort to complete this study with a scientific background. The manuscript is written in a good manner and organized properly, however, there are a few corrections to be made before acceptance of the manuscript which is explained in detail below. The title reflects the main subject of the manuscript but there is a mismatch between the title of this manuscript and the study registered in Prospero (study number: CRD42022354620). The abstract summarizes the described work. Sufficient keywords are provided and the introduction covers adequate background information but still, the significance of this study needs to be addressed in brief. The method of source retrieval, inclusion, and exclusion criteria are mentioned adequately. The results are discussed in detail and they can serve as a source for further research in this field. The discussion part is elaborate and can be made more concise and clearer. Illustrations and tables are provided adequately, but the referring of the figures in the context is still not properly done. The author has used the proper biostatistics, units, and references. The quality of the manuscript is good and follows PRISMA 2009 Checklist.

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Peer-review model: Single blind

Reviewer's code: 06118161
Position: Editorial Board
Academic degree: FACS, FICS, FRCS (Ed), MBBS, MS
Professional title: Professor
Reviewer's Country/Territory: India
Author's Country/Territory: China

Manuscript submission date: 2022-12-19

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Reviewer accepted review: 2023-01-28 06:22

Reviewer performed review: 2023-01-28 15:50

Review time: 9 Hours

Scientific quality	[ ] Grade A: Excellent [ ] Grade B: Very good [Y] Grade C: Good [ ] Grade D: Fair [ ] Grade E: Do not publish
Novelty of this manuscript	[ ] Grade A: Excellent [Y] Grade B: Good [ ] Grade C: Fair [ ] Grade D: No novelty
Creativity or innovation of this manuscript	[ ] Grade A: Excellent [Y] Grade B: Good [ ] Grade C: Fair [ ] Grade D: No creativity or innovation

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Scientific significance of the conclusion in this manuscript	[ ] Grade A: Excellent [Y] Grade B: Good [ ] Grade C: Fair [ ] Grade D: No scientific significance
Language quality	[Y] Grade A: Priority publishing [ ] Grade B: Minor language polishing [ ] Grade C: A great deal of language polishing [ ] Grade D: Rejection
Conclusion	[ ] Accept (High priority) [Y] Accept (General priority) [ ] Minor revision [ ] Major revision [ ] Rejection
Re-review	[ ] Yes [Y] No
Peer-reviewer statements	Peer-Review: [Y] Anonymous [ ] Onymous
	Conflicts-of-Interest: [ ] Yes [Y] No

SPECIFIC COMMENTS TO AUTHORS

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PEER-REVIEW REPORT

Name of journal: *World Journal of Gastroenterology*

Manuscript NO: 82456

Title: Different types of fruit intake and colorectal cancer risk: a meta-analysis of observational studies

Provenance and peer review: Unsolicited Manuscript; Externally peer reviewed

Peer-review model: Single blind

Reviewer's code: 06482162
Position: Peer Reviewer
Academic degree: N/A

Professional title: N/A

Reviewer's Country/Territory: Taiwan

Author's Country/Territory: China

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Reviewer accepted review: 2023-01-07 04:21

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Review time: 21 Days and 21 Hours

Scientific quality	[ ] Grade A: Excellent [ ] Grade B: Very good [ ] Grade C: Good [Y] Grade D: Fair [ ] Grade E: Do not publish
Language quality	[ ] Grade A: Priority publishing [Y] Grade B: Minor language polishing [ ] Grade C: A great deal of language polishing [ ] Grade D: Rejection
Conclusion	[ ] Accept (High priority) [ ] Accept (General priority) [ ] Minor revision [Y] Major revision [ ] Rejection
Re-review	[Y] Yes [ ] No

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

SPECIFIC COMMENTS TO AUTHORS

First of all, I would like to thank the editors for granting me this opportunity to review this interesting meta-analysis. In my opinion, a major revision is required though it is a potentially acceptable manuscript. My comments on the manuscript are as follows. Major Comments: 1. Although authors' searching strategy seems to be thorough, I would advise to include flavonoid and nobiletin, two of the main compounds in fruit possessing anti-cancer ability in colorectal cancers into the keyword to include more potentially eligible articles to avoid publication bias. 2. In the section of statistical analysis, I strongly disagree with the use of the fixed-effects model even if the I2 is less than 50%. The reason is plain and simple: for observational studies, it is inevitable to encounter conceptual heterogeneity even if there is no statistical heterogeneity, especially in observational studies where the assumption that all studies estimate the same underlying effect is rarely justified as population characteristics, exposure, and outcome definitions are highly likely differ across studies. Therefore, using a random-effects model for combining observational studies seems a lot more reasonable. All of which are clearly indicated in the latest Cochrane Handbook for Systematic Reviews of Interventions. As a result the sentence of "Significant heterogeneity was considered when $I^2>50\%$ and $p<0.05$, and a random-effects model was used, otherwise a fixed-effects model was employed" in the section of statistical methods should be revised. 3. For assessing non-RCT study, I suggest authors should use ROBINS-I ("Risk Of Bias In Non-randomised Studies - of Interventions") for cohort studies instead of Newcastle-Ottawa Scale, which is considered outdated after the advent of ROBINS-I. 4. As performing a sensitivity analysis based on quality assessment is a common

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action and is not associated with selection bias, I advise authors should perform sensitivity analyses based on quality assessment and should delete the following sentence in the section of assessment of study quality in the main text: "To avoid selection bias, no studies were excluded due to these quality criteria." 5. In authors' meta-analysis, I would suggest excluding cross-sectional studies because study participants are assessed at a specific time point and the temporal relationship between exposure and outcome can often not be determined. 6. Although it is understandable to use adjusted OR/RR for meta-analysis and it is very informative and applaudable to present confounding factors in Table 1, I would suggest authors to also present meta-analysis of unadjusted OR/RR because confounding factors that were adjusted in each study were not identical, which can potentially give rise to a source of between-study variance. 7. Dose response meta-analysis seems solid and sound. 8. Can authors elaborate more on how they attain OR/RR in Table 1? Take Lin et al. 2005 for example, I have a hard time finding the OR of 1.11 (0.71-1.74) in the original paper and I would like to gently ask authors to shed more light on it. Minor Comments: 1. PROSPERO should be spelled out all in capital. 2. English writing should be edited by a native speaker.

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Provenance and peer review: Unsolicited Manuscript; Externally peer reviewed

Peer-review model: Single blind

Reviewer's code: 03806663
Position: Editorial Board
Academic degree: MD
Professional title: Professor
Reviewer's Country/Territory: Egypt
Author's Country/Territory: China

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Reviewer performed review: 2023-02-01 18:04

Review time: 3 Days and 20 Hours

Scientific quality	[ ] Grade A: Excellent [ ] Grade B: Very good [Y] Grade C: Good [ ] Grade D: Fair [ ] Grade E: Do not publish
Novelty of this manuscript	[ ] Grade A: Excellent [Y] Grade B: Good [ ] Grade C: Fair [ ] Grade D: No novelty
Creativity or innovation of this manuscript	[ ] Grade A: Excellent [Y] Grade B: Good [ ] Grade C: Fair [ ] Grade D: No creativity or innovation

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Scientific significance of the conclusion in this manuscript	[ ] Grade A: Excellent [Y] Grade B: Good [ ] Grade C: Fair [ ] Grade D: No scientific significance
Language quality	[ ] Grade A: Priority publishing [Y] Grade B: Minor language polishing [ ] Grade C: A great deal of language polishing [ ] Grade D: Rejection
Conclusion	[ ] Accept (High priority) [ ] Accept (General priority) [Y] Minor revision [ ] Major revision [ ] Rejection
Re-review	[ ] Yes [Y] No
Peer-reviewer statements	Peer-Review: [Y] Anonymous [ ] Onymous
	Conflicts-of-Interest: [ ] Yes [Y] No

SPECIFIC COMMENTS TO AUTHORS

It is an exciting article that discusses a hot issue. I have comments: is it easy to know which type of fruit is beneficial for the prevention of CRC.? Also, is it easy to know which component in these fruits works against cancer and which can promote ulcers? And is it a causal effect relationship or just an association? what is the amount of fruit needed, and for how long?

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RE-REVIEW REPORT OF REVISED MANUSCRIPT

Name of journal: *World Journal of Gastroenterology*

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Title: Different types of fruit intake and colorectal cancer risk: a meta-analysis of observational studies

Provenance and peer review: Unsolicited Manuscript; Externally peer reviewed

Peer-review model: Single blind

Reviewer's code: 06482162
Position: Peer Reviewer
Academic degree: N/A

Professional title: N/A

Reviewer's Country/Territory: Taiwan

Author's Country/Territory: China

Manuscript submission date: 2022-12-19

Reviewer chosen by: Yu-Lu Chen

Reviewer accepted review: 2023-03-03 09:25

Reviewer performed review: 2023-03-04 07:22

Review time: 21 Hours

Scientific quality	[ ] Grade A: Excellent [Y] Grade B: Very good [ ] Grade C: Good [ ] Grade D: Fair [ ] Grade E: Do not publish
Language quality	[Y] Grade A: Priority publishing [ ] Grade B: Minor language polishing [ ] Grade C: A great deal of language polishing [ ] Grade D: Rejection
Conclusion	[ ] Accept (High priority) [Y] Accept (General priority) [ ] Minor revision [ ] Major revision [ ] Rejection
Peer-reviewer	Peer-Review: [Y] Anonymous [ ] Onymous

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statements	Conflicts-of-Interest: [ ] Yes [Y] No

SPECIFIC COMMENTS TO AUTHORS

The authors have revised their manuscript appropriately, significantly enhancing the quality of their article. I would like to congratulate authors on their significant contribution. However, I regret that I still disagree with the authors using NOS as the mainstay tool for quality assessments. According to Cochrane Handbook Chapter 25, ROBINS-I is recommended when assessing non-RCT studies. The underlying rationale for its use resides in the fact that the idea evaluations of non-RCT studies are facilitated by attempting to emulate a hypothetical pragmatic randomized trial. This is of utmost importance as researchers are passionate for the possible use of non-RCT studies to provide evidence with regard to the comparative effectiveness of given interventions because conductions of RCT are expensive, time consuming, and may not reflect real world experience with healthcare interventions (Ann Intern Med2009;151:203-5). Of note, NOS tool is not designed to evaluate non-RCT studies in this manner so it is main reason why it is considered outdated with the advent of ROBINS-I. I speculate that what the authors referred to regarding the low usability of ROBINS-I is a conclusion from a recent paper by Zhang et al. (J Evid Based Med. 2021;1–11.) suggesting that appraising studies with the use of ROBINS-I is time-consuming and advanced training in epidemiology is mandatory, which is true as it would be difficult to use it without experts in both methodology and subject-related content. However, it should not be the reason against using it. Although it may not affect the ultimate meta-analysis results regardless of using NOS or ROBINS-I in this study, it would be more appropriate to use ROBINS-I than otherwise in terms of solid and sound methodology and I still suggest authors revise the quality assessment section in accordance with the strictest methodology.