

## Peer Review File

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### Reviewer A

This is a poor meta-analysis. First of all, the authors focused on the differences in obesity parameters in NAFLD and non-NAFLD children and the effectiveness of exercise for NAFLD. However, the authors only provided the methodology details for the first focus, but did not report any details for the second focus. In general, one research should answer one research question, so I suggest the authors to delete the second focus. The title is also misleading because the authors did not collect data on the diagnosis of obesity in both the NAFLD and non-NAFLD groups.

Reply: Thank you for your suggestion. The study focused on the first research question, "Differences in indicators of obesity-related parameters in children with NAFLD and NASH" situation. The second question was not the primary focus of this study, so in the title, the content related to the second question was removed from the title to avoid being misleading. Secondly, because of the variety of forms of exercise, it is difficult to specifically go into the details of the second research question to quantify it specifically.

Changes in the text: Paragraph 1/ Title

Second, the introduction of the main text is very bad. In general, meta-analysis is used to address clinical controversy but the authors did not provide any examples on the conflicting findings on the obesity parameters in NAFLD and non-NAFLD children, did not analyze the potential reasons for the controversy, and did not explain why a meta-analysis is suitable to address the controversy. In this part, the significance was described as "to establish a reference range of obesity related parameters for the early screening of NAFLD in children", however, this meta-analysis cannot provide such reference range, as well as cannot provide data on the accuracy of these parameters for the screening of NALLD.

Reply: Thank you for your suggestion. In the introductory section, the paper states that "a large number of studies have reported the association between obesity and childhood NAFLD, but there is still a lack of reasonable scientific screening methods for NAFLD in obese children", which is the significance of this meta-analysis, which addresses the conflict "lack of reasonable screening methods for the diagnosis of NAFLD in overweight children"

Changes in the text: Paragraph 3 /Introduction

Third, in the methodology of the main text, the publication dates of studies to be included should not be limited to "January 2011 and December 2022" only, which would result in selection bias. The search within CNKI for Chinese-language literature is also inadequate because CNKI does not cover all Chinese-language literature. In the inclusion criteria, there is no "case-control experiments" in clinical epidemiology. The inclusion of "clinical trials" is incorrect for this meta-analysis of comparative studies. In the quality assessment, the score of 6-9 for high-quality studies is not convincing. The authors need to provide supporting

references. In statistics, the authors need to further explain procedures for “logistic regression was first used to calculate the effect size of each included study and its standard error” because in the above inclusion criteria, NAFLD is a binary outcome, the presence or absence. The authors need to describe the test of sources of heterogeneity and the test of the influence of quality on the pooled results.

The paper needs to be reviewed again after extensive revisions.

Reply: Thank you for your suggestion. The article focuses mainly on relevant studies from the last 10 years, too far back in time to be of value for inclusion in our opinion because the measurement methods for the various parameters have improved over time. We only searched CNKI database for Chinese articles, because we searched other databases at that time and found that articles were duplicate. This shortcoming have be added to the limitations of the article in the discussion section. The inclusion criteria have been changed to 'original studies' at the time of writing, as this was not accurately stated. In the Methods section, the significance of regression analysis has been added in detail.

Changes in the text: Paragraph 3,8 / Discussion, Paragraph 2,7 / Methods

## **Reviewer B**

1. This revised version is still rather poor. First, the title did not indicate “children” and the title is not consistent with the research work of this study, obesity-NAFLD relationship in the title vs. obesity-related indicators between NAFLD and non-NAFLD participants in the main text. The two are different research questions.

Reply: Thank you for your reminder. We accidentally deleted the word "children". The title has now been changed to “Relationship between obesity related indicators and non alcoholic fatty liver disease in children: a meta-analysis”

Changes in the text: Paragraph 1/ Title

2. The authors described that they deleted the second focus on exercise but the abstract and the main text including the results and tables still had data on the exercise.

Reply: Thanks for your comment. Exercise is not the main analysis result, so it has been removed from the title. However, as a secondary result, it is still included in the abstract and text in order to enrich the article.

Changes in the text: None.

3. The abstract is still problematic, which did not describe the controversy to be addressed in the background, did not describe the inclusion of studies and the risk of bias assessment of included studies in the method, did not describe the total numbers of cases and controls and the level of risk of bias of included studies, and the conclusion had no comments for the clinical implications of the findings. Importantly, the authors should revise the whole paper accordingly, not only limiting to the parts I commented.

Reply: Thanks for your suggestion. We have modified the abstract and revised the whole paper

accordingly based on your suggestions.

Changes in the text: Paragraph 1-4 /Abstract; Paragraph 1-3 /Introduction; Paragraph 8 / Discussion

4. In the introduction, I still did not find the clinical controversy to be addressed by this meta-analysis and why a meta-analysis is suitable for addressing it. The rationales were described as “a standard, evidence-based screening method for NAFLD in obese children is still lacking” and “providing the development of reference ranges for overweight-related parameters for early screening of NAFLD in children”. This is very bad and wrong since for screening the authors should provide data on the screening accuracy of the obesity-related parameters for NAFLD, but the authors only pooled the differences by using MD values, which cannot provide data on “reference ranges”. The authors must rewrite the paper all throughout, in particular the introduction part.

Reply: Thanks for your suggestion. Some of the content in the introduction is indeed inappropriate. We have reworded the introduction.

Changes in the text: Paragraph 3 /Introduction

5. In the methodology of the main text, the term “PRISMA regulations” is wrong. The authors’ explanations on limiting publication dates to “2011-2022” and limiting the search of Chinese-language papers to CBKI only are not convincing and obeyed the basic principles of systematic review. The authors should correct these according to the standard methodology guidelines of systematic review.

Reply: Thanks for your comments. We have changed the publication date to before 2022. The search scope of Chinese literature has been expanded to CNKI, Wanfang and VIP databases.

Changes in the text: Paragraph 1 /Methods

6. The inclusion criteria are still not clear and problematic, which did not strictly define according to the PICOS principles. “original research” is not a subtype of clinical research. In quality assessment, the 6-9 NOS score was regarded as high-quality studies is also problematic. In statistics, the sentence “As fatty liver disease was a categorical ordinal variable, and therefore differs from a dichotomous variable such as cure/exacerbation of disease, we were not able to calculate the combined effects directly using Stata/SE 16.0 software to calculate the relative risk. Logistic regression was first used to calculate the effect size of each included study and its standard error, followed by the generic inverse variance method in Review Manager 5.4 to combine the effect size for meta-analysis” is misleading and wrong since in the authors’ results NAFLD is binary outcome variable.

Reply: Thanks for your comments. We have revised the inclusion criteria for inclusion in the literature in accordance with the PICOS principles. In the method section, we have modified the description of the evaluation of the NOS scale included in the study quality. NAFLD is the ordinal variable for classification only when studying the effect of exercise on the improvement of nonalcoholic fatty liver disease in children. Therefore, only when analyzing the improvement effect of exercise on nonalcoholic fatty liver disease in children, we first used logistic regression, and then combined the relevant effects.

Changes in the text: Paragraph 2,6 /Methods

7. The discussion part should be rewritten after the revisions of the methodology.

Reply: Thanks for your comments. The discussion section has been revised.

Changes in the text: Paragraph 1,5,8 / Discussion