

Title: Geographic variation and temporal trends in adolescent obesity prevalence in India: A systematic review and meta-analysis

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Reviewer C: Anonymous, COI: None, AI disclosure: None

Overview

This manuscript provides a substantial and methodologically strong systematic review and metaanalysis of adolescent obesity in India, pooling 60 cross-sectional studies across 24 states. It shows that obesity prevalence is 7.2%, is rising over time, varies geographically, and is strongly influenced by the BMI classification standard used, with WHO criteria appearing most consistent. As a reviewer, I find the topic highly relevant, the dataset broader than prior reviews, and the subgroup and temporal analyses valuable. The manuscript is generally well organised, informative, and potentially publishable, though some methodological and presentation refinements may still strengthen it.

- 1. Comment** I find the title somewhat ambitious in relation to the evidence base. The manuscript is based entirely on cross-sectional studies, so the phrase “temporal trends” may overstate the strength of the inference, since the analysis reflects changes across published studies over time rather than true longitudinal surveillance of the same population. A more cautious wording such as “temporal patterns” or “changes over study years” may be more methodologically precise. In addition, the title could be slightly shortened for clarity and impact.

Author response: All included studies are cross-sectional, the manuscript now states (Discussion, 4) that “trend” and “rising burden” denote comparisons across cohorts at different time points, and the meta-regression is described as suggestive. The registered title is retained for indexing consistency, with all interpretive statements softened accordingly.
- 2. Comment** I see several points that could be improved. First, the abstract is somewhat dense and overloaded with statistical detail, which may reduce readability. Not every metric shown in the main results section needs to appear in the abstract. Second, the claim regarding “temporal trends” should be phrased more cautiously, because the analysis is based on cross-sectional studies conducted over different years rather than longitudinal follow-up data. Third, the conclusion is slightly stronger than the evidence in places, especially the recommendation that WHO BMI standardization is “urgently needed,” which may be reasonable but should be framed as an implication rather than a definitive result. Finally, the abstract would benefit from a brief mention of key limitations, particularly the high heterogeneity and the cross-sectional nature of all included studies. Overall, the abstract is informative and mostly accurate, but it would be stronger if it were slightly more concise, more cautious in interpretation, and more transparent about limitations.

Author response: The Conclusion has been shortened, the WHO-standardisation statement reframed as a policy implication, and the cross-sectional design and high heterogeneity are now noted as explicit limitations; temporal wording is more cautious.
- 3. Comment** The objectives are somewhat ambitious in relation to the nature of the available evidence. Because all included studies are cross-sectional, the objective of assessing “temporal trends” may be methodologically stronger than the data can fully support. A more cautious formulation such as examining temporal patterns across study years would be more precise. In addition, while the objectives are comprehensive, they could be stated more concisely to improve readability and focus.

Author response: The objective is now interpreted as examining temporal patterns across study years, consistent with the cautionary language in the Design and Discussion sections.
- 4. Comment** The rationale could be made more concise and focused. Parts of the Introduction are quite detailed and at times read more like an extended background review than a sharply argued justification for the present study. In addition, the manuscript could distinguish more explicitly between what is truly unknown and what is already established in prior reviews. The claim that earlier meta-analyses were inadequate is reasonable, but it would be stronger if the exact methodological or evidence gaps were presented even more directly and comparatively.

Author response: The Introduction now foregrounds the specific gaps addressed — expanded pool ($k = 60$), first six-region geographic analysis, and formal temporal analysis — relative to earlier reviews.
- 5. Comment** I would note that some operational details could still be presented more explicitly in the main text or appendix, particularly the full search strategy, screening process implementation, and handling of Google Scholar results for maximum transparency. Regarding ethics, the manuscript does not appear to raise major ethical concerns because it is a systematic review and meta-analysis of published studies, but this should still be stated explicitly. A brief sentence clarifying that ethical approval was not required because no primary human data were collected would improve completeness.

Author response: A reproducible Google Scholar protocol was added (§2.2; private session, first 200 records per query, independent re-run, $\kappa = 0.91$). As a secondary synthesis of published aggregate data, ethical approval was not required; this is stated explicitly.
- 6. Comment** The design is more convincing for estimating prevalence than for supporting stronger inferences about “temporal trends.” Because all included studies are cross-sectional and drawn from different populations and settings, the design can only assess temporal patterns across study years rather than true longitudinal change. In addition, the substantial heterogeneity and variation in BMI classification methods limit the strength of pooled comparisons. Overall, the design is appropriate and

well chosen for most objectives, but some claims should be framed more cautiously to match the limits of the underlying study designs.

Author response: The temporal interpretation now notes three confounders (WHO drift, urbanisation, sampling frames) and states the trend is suggestive rather than conclusive without longitudinal data.

- 7. Comment** The description could still be strengthened in a few areas. The rationale for selecting specific analytical choices, particularly the handling of substantial heterogeneity and the interpretation of publication bias, could be explained more fully. In addition, although the methods are statistically sound overall, the manuscript should present greater caution in linking meta-regression results to temporal “trends,” given the cross-sectional and heterogeneous nature of the included studies. Overall, the statistical methods are appropriate and reasonably comprehensive, but some aspects would benefit from clearer justification and more cautious interpretation.

Author response: Reliability under high heterogeneity is justified by three checks (WHO-restricted analysis $I^2 = 30.0\%$; leave-one-out $7.0\%–7.3\%$; prediction interval $4.1\%–12.4\%$). Trim-and-fill was applied (adjusted 7.5%), supporting heterogeneity rather than publication bias as the source of funnel asymmetry.

- 8. Comment** The tables could be improved in terms of clarity and presentation. Some tables are text-heavy and contain dense information that may be difficult to scan quickly, especially for readers trying to identify the main findings at a glance. The formatting could be streamlined to improve readability, and certain explanatory notes might be better moved to footnotes rather than placed directly within the body of the tables. In addition, some tables would benefit from clearer emphasis on the most important comparative results rather than presenting all details with equal visual weight. Overall, the tables are appropriate and useful, but modest refinement in layout and simplification would improve their clarity and impact.

Author response: The subgroup tables were merged into a single Table 3 with three panels, all tables collated in citation order, and explanatory material moved to footnotes.

- 9. Comment** The clarity of some figures could be improved. Several forest plots appear information-dense, with many study rows and statistical details, which may make them difficult to read quickly in article format. The captions are generally informative, but some figures would benefit from simplified presentation or stronger visual emphasis on the main message rather than displaying all elements with equal prominence. In addition, where tables already provide extensive numeric detail, some figures may partially duplicate information rather than offering a more interpretive visual summary.

Author response: Redundant subgroup forest plots were removed because the same data appear in Table 3, leaving the PRISMA diagram and the primary forest plot as the main-text figures.

- 10. Comment** The manuscript often repeats numerical findings in full within the narrative even when the same data are already presented clearly in the corresponding tables and forest plots. This is particularly evident in the reporting of pooled prevalence values, subgroup estimates, and heterogeneity statistics, where the text sometimes restates results rather than focusing on interpretation and the most important contrasts. The Results section would be stronger if the authors reduced repetition and used the text more selectively to highlight key findings, patterns, and unexpected observations while leaving detailed numerical reporting primarily to the tables and figures.

Author response: The Results narrative was tightened to highlight key contrasts, with detailed values carried by tables and figures.

- 11. Comment** The section is somewhat overextended and does not fully avoid repetition of the Results. Restating several numerical findings and subgroup estimates in detail before interpreting them reduces the distinct role of the Discussion. In addition, some interpretations are stronger than the observational evidence can firmly support, particularly around temporal inference and policy recommendations.

Author response: Interpretive claims were softened and duplicated numerical reporting removed; the Discussion now emphasises interpretation.

- 12. Comment** The limitations could be discussed more explicitly in relation to how they affect interpretation. In particular, the implications of high heterogeneity, possible small-study effects, and the limits of inferring temporal “trends” from repeated cross-sectional evidence deserve stronger emphasis.

Author response: Limitations now address school-based predominance (offset by school- vs community-based agreement, 7.0% versus 6.9%), the exploratory Central/Northeast subgroups ($k = 3$), and the limits of cross-sectional temporal comparison.

- 13. Comment** Some parts of the conclusion are stated more strongly than the evidence fully allows. In particular, the recommendation for urgent WHO BMI standardization and the interpretation of a rising burden over time should be framed more cautiously, because the evidence is based entirely on heterogeneous cross-sectional studies rather than longitudinal surveillance data. Similarly, policy implications are reasonable, but they should be presented as informed recommendations rather than definitive conclusions directly proven by the analysis.

Author response: The Conclusion is now a two- to three-line statement framed as an informed recommendation, with cautious temporal wording.

- 14. Comment** There are some concerns. The reference list appears to contain duplicate entries, which raises questions about editorial accuracy and reference management. In addition, while the references are largely relevant, the manuscript would benefit

from ensuring that all cited sources are presented consistently and that the most recent Indian and global evidence is used selectively rather than extensively.

Author response: The reference list has been renumbered into strict serial (Vancouver) order and the citation columns of Tables 1 and 2 corrected. The duplicate entries flagged have been identified and will be reconciled in the production file.

- 15. Comment** The storytelling is not consistently concise. At several points, particularly in the Introduction and Discussion, the narrative becomes overly dense and statistically crowded, which weakens readability and reduces the impact of the key messages. Some arguments are repeated in slightly different forms, and a more streamlined presentation would make the manuscript sharper and more compelling. Overall, the story is logical and understandable, but it would benefit from tighter writing and clearer emphasis on the central take-home points.

Author response: The Introduction and Discussion were tightened and the Conclusion shortened; the main text is now $\approx$ 3,720 words.

- 16. Comment** The paper is somewhat verbose in places. The Introduction and Discussion are longer than necessary and occasionally repeat points that could be stated more succinctly. Some interpretations are also expressed with more detail than needed, which affects the overall sharpness of the manuscript. A modest reduction in length through tighter writing, especially in the background and discussion sections, would likely improve readability without compromising substance. Overall, the article length is acceptable, but selective condensation would strengthen clarity and presentation.

Author response: The manuscript was condensed, principally in the Introduction, Discussion and Conclusion.

- 17. Comment** The language would still benefit from careful editing before publication. At several points, the prose is overly dense, with long sentences and heavily packed statistical details that reduce readability. Some statements sound more assertive than necessary for observational evidence, particularly in the Discussion and Conclusion, where interpretations occasionally go beyond a cautious academic tone. There are also places where wording could be simplified for clarity and conciseness, and a few sentences would benefit from polishing for flow and precision.

Author response: The prose was edited for concision and flow (Grammarly used for routine checking, per the AI Disclosure), and over-assertive statements moderated.

Reviewer D: Sharmin Mahbuba, ORCID: 0000-0002-5802-3542, COI: None, AI disclosure: None

Overview

The manuscript explores an important public health issue by evaluating the geographic variation and temporal trends in adolescent obesity prevalence in India. The study contributes valuable region-specific evidence from a high-prevalence setting. The topic is relevant to current burning issue of non-communicable disease burden on epidemiological background. However, several methodological clarifications, statistical reporting improvements, and language revisions are required before the manuscript can be considered for publication.

- 18. Comment** It is clear and logically structured as it has been stated that this study would include 24 states of 6 regions of India to find out the geographical variation of adolescent obesity prevalence and would include 5 categories of classification of obesity based on BMI. Also as previous meta analysis included less study pools this study appropriately included a larger study pool. Yet the author did not mention how this meta analysis study would help like in measuring disease burden, in creating awareness or in prevention of complications.

Author response: The Discussion now states the applied value: national burden quantified ($\approx$ 18.2 million adolescents), regional analysis identifies priority areas for school-based prevention, and the economic framing (costs to 1.5% of GDP by 2060) motivates early investment.

- 19. Comment** This study was registered in PROSPERO, PRISMA was used to ensure comprehensive and transparent reporting and conducted under JBI review guidance. The things lacking are whether independent double coding and Newcastle Ottawa scale were used. Also there was no declaration for the funding source or conflict of interest.

Author response: Selection and extraction were independent ($\kappa = 0.91$). Quality was appraised with the JBI tool for prevalence studies (the standard instrument), not Newcastle–Ottawa; this is stated in §2.6. Funding and Conflict-of-Interest statements have been added (no specific funding; no conflicts).

- 20. Comment** The cross sectional study design was appropriate. The figure 1 is almost clear to describe the study but needs certain corrections (find the attached file)

Author response: The PRISMA 2020 diagram was checked against the reported counts (2,847 $\rightarrow$ 1,384 duplicates $\rightarrow$ 1,463 screened $\rightarrow$ 153 full texts $\rightarrow$ 60 included); specific annotated corrections will be applied.

- 21. Comment** Statistical methods are elaborately described. Appropriate statistical methods were used for meta analysis, grade certainty assessment, to avoid publication bias. Temporal trend was measured by meta-regression (Could not find the appendix Figure A2, Figure A4 and Figure A5).

So need some corrections (find the attached file).

Author response: The appendix figures (A1 leave-one-out; A2 funnel/Egger; A3 setting forest plot; A4 cumulative meta-analysis; A5 temporal meta-regression) are provided in the Supplementary Material and cross-referenced by number; we have confirmed their inclusion and labelling.

- 22 Comment** Tables are clear and appropriate. Needs few corrections (Find the suggestions at attached file). In some cases footnotes to be added.
- Author response:** Tables were consolidated and footnotes added (e.g., the GRADE key beneath Table 4); annotated corrections will be applied.
- 23. Comment** The figure 1 clearly tells about the study selection process. Few corrections are advised for figure 1 in attached file. Could not find difference between figure 2 and 3. Figure 2 can be omitted. I did not find appendix figure A1, Figure A2, Figure A4, Figure A5, figure 4b (appendix A3) in the manuscript downloaded, please clarify.
- Author response:** The redundant subgroup figure was removed, resolving the duplication, and figures renumbered; appendix figures are in the Supplementary Material (see B7).
- 24 Comment** No major redundancy between text and tables/figures were found. The evidence of some text were not found in the downloaded manuscript which is already mentioned (appendix figure A1, Figure A2, Figure A4, Figure A5, figure 4b (appendix A3) were not found)
- Author response:** See B7/B9 — the appendix figures are provided in the Supplementary Material and cross-referenced.
- 25. Comment** The discussion justifies the main message of the manuscript by comparing the current study's findings with the scientific literatures (referencing Popkin, Misra, and the NCD Risk Factor Collaboration). It didn't just report the study; it also provided recommendation like WHO criteria alone achieve sufficient homogeneity for meaningful pooling suggesting that WHO z-scores already perform adequately for Indian adolescents without requiring a separate national reference. The temporal trend (increasing by 2.3 million adolescents over the decade) is justified by linking it to economic costs (1.5% of GDP by 2060) and adult projections.
- Author response:** Justifies the main message; useful recommendations.
- 26 Comment** Though the study is strong in structure, statistical methods and described in text, limitations were not clearly written. It would be better if the strength and limitations were described in a separate paragraph having this heading.
- Author response:** Limitations are now explicit (cross-sectional design, English-language restriction, Central India under-representation, school-based predominance, no individual-participant data); we are glad to present them under a dedicated heading.
- 27. Comment** Conclusion of this study supports the data found in result but it should be concise. (Follow the suggestion in attached file)
- Author response:** The Conclusion was shortened to a two- to three-line statement.
- 28 Comment** Maximum reference are up-to-date and appropriate. Better to avoid reference more than 10 years old.
- Author response:** The reference base was reviewed and updated; a few older references are retained as primary methodological or seminal sources (foundational heterogeneity/funnel-plot methods and the Indian reference standards evaluated).
- 29. Comment** The length of the article is almost appropriate. It can be curtailed as suggested in the attached file.
- Author response:** The manuscript was condensed ($\approx 3,720$ words), primarily in the Introduction and Discussion.
- 30. Comment** Several sentences require grammatical correction. Provide abbreviation at the beginning of use.
- Author response:** The manuscript was edited for grammar and flow, and all abbreviations are defined at first use.
- Responsible editor:** Tahniyah Haq, ORCID: 0000-0002-0863-0619
- 31. Comment** Please include a section "Key message" after the abstract. This should not be more than 60 words.
- Author response:** A Key Message paragraph of 49 words has been inserted immediately after the Keywords
- 32. Comment** Please shorten the conclusion to one or two lines outlining the main finding.
- Author response:** Conclusion has been reduced from a 240-word paragraph to a 56-word, three-line summary that states only the main finding and the policy recommendation
- 33. Comment** Tables and figures: total ≤ 6 and placed at end of manuscript.
- Author response:** The previous version contained 6 tables (Tables 1, 2, 3a, 3b, 3c, 4) and 4 main-text figures (Figures 1, 2, 3, 4a) for a total of 10 items. The revised version contains 4 tables and 2 figures (total = 6), all placed in a new, consolidated "Tables and Figures" section immediately before the References.
- 34. Comment** Reduce the manuscript word count ≤ 5000 (excluding tables, figures, references).
- Author response:** The main manuscript (Title, Abstract, Key Message, Introduction, Methods, Results narrative, Discussion, Conclusion) now contains approximately 3,720 words, comfortably below the 5,000-word ceiling. Tables, figures, table/figure captions and References were excluded from the count, in line with the journal's policy.

35. Comment Why was the search limited to December 2024 rather than 2026?

Author response: The search end-date of 31 December 2024 was not arbitrary — it was prospectively defined at PROSPERO registration (CRD420251168601) to align with the protocol-specified analytic window of 2013–2024, in compliance with PRISMA 2020 (Page et al., 2021), PRISMA-S (Rethlefsen et al., 2021), and JBI prevalence-review guidance (Munn et al., 2020), which require the search cut-off to be locked at registration so that study identification cannot be influenced by interim results. Extending the cut-off into 2025–2026 was considered but rejected for three methodological reasons: (i) it would constitute a post-hoc protocol amendment requiring re-registration and a complete re-screen, shifting the analytic focus from a clean decadal snapshot to an incomplete partial year; (ii) indexing in PubMed, MEDLINE, Embase, and Google Scholar typically lags publication by 6–12 months, so any cut-off in early 2026 would yield a biased and incomplete sample of late-2025 and 2026 literature; and (iii) the registered window already produced an unusually large evidence base (60 cross-sectional studies; n=36,391 adolescents across 24 Indian states), with sufficient statistical power, geographic coverage, and temporal range to address all prespecified objectives. To make this rationale explicit, a new justification paragraph has been added at the end of Methods Section 2.2 and is highlighted in yellow in the resubmitted manuscript for the editor's ease of review. The original 31 December 2024 cut-off, the 2,847-record search yield, and the 60-study analytic dataset are therefore retained without change.