PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Neighborhood socioeconomic status and overweight/obesity: a systematic review and meta-analysis of epidemiological studies
AUTHORS	Mohammed, Shimels Hussien; Habtewold, Tesfa; Birhanu, Mulugeta; Sissay, Tesfamichael Awoke; Tegegne, Balewgizie; Abuzerr, Samer; Esmaillzadeh, Ahmad

VERSION 1 – REVIEW

REVIEWER	Michelle Wong U.S. Department of Veterans Affairs USA
REVIEW RETURNED	15-Jan-2019

GENERAL COMMENTS	Overview: In this paper, the authors synthesize through a meta-analysis the association between neighborhood socioeconomic status and overweight/obesity. The authors note that while there has been significant interest in research in this area, there has yet to be a comprehensive meta-analysis of these findings. Their study fills this gap, which has implications for addressing the obesity epidemic through broader policies beyond individual-level behavior change. The authors conduct a comprehensive review of the literature, use methods that are appropriate for meta-analyses, and their conclusions are reasonable given the results. Specific comments: Introduction: 1. The authors switch between several terms: neighborhood socioeconomic status (NSES), neighborhood deprivation, and poor neighborhoods. It would be helpful if the authors chose one of these terms and used it consistently throughout the manuscript. 2. The introduction could be strengthened by more discussion about potential mechanisms linking NSES to obesity beyond just aspects of the built environment. 3. Related to this, there are a variety of ways of conceptualizing NSES, beyond just poverty, and this is important for the proposed mechanisms linking NSES to obesity (e.g., resources, education, collective norms and collective efficacy, social norms). It is unclear how the authors are considering NSES. Methods 4. Page 7, line 9: what do the authors mean by "from inception"? What is the start date range for the search period? 5. It would be helpful to know a bit more about what criteria the Newcastle-Ottawa Scale uses to evaluate the study quality

Results
6. I am surprised by how few studies were included in the meta-analysis. My impression of the literature is that more studies have assess the relationship between NSES and obesity. Would be helpful to know more about the studies that were included and studies that were not included in the meta-analysis. For example, did the authors consider studies that looked at a variety of neighborhood exposures, one of which is NSES? What about studies that came out of Moving to Opportunity (e.g., Chetty 2016 article that the authors cite)?
7. Results from the assessment of study quality is not included in the Results.
8. The authors distinguish between whether the studies included in the meta-analysis used NSES or NDI in Table 1, but do not provide any information about how they are distinguishing between these two terms
9. Related to this even within NSES and NDI, there is significant heterogeneity in how these indices are created (e.g., what factors go into the indices) and how they are measured and operationalized in the regression models (e.g., continuous measure, quartiles/quintiles/deciles, comparison of highest vs. lowest…). It would be helpful to have a sense of how the studies measuring NSES and NDI.
10. In general, it would be helpful to have more summary information about the studies. E.g., what characteristics did the studies control for?

Discussion
11. The difference in the pooled findings between the cross-sectional and longitudinal studies is interesting and warrants further discussion, particularly the lack of association in the longitudinal studies.
12. It seems like a randomized controlled study to assess the effects of NSES on obesity would be challenging/nearly impossible, with both practical and ethical issues. How do the authors envision such a study? Or perhaps the authors could propose other study types that can assess causality that are stronger than the numerous extant cross-sectional studies?
13. The authors mention that obesity interventions have focused on providing information to address the individual-level determinants. However, there have been efforts to improve health promoting resources in low NSES communities e.g., built environment changes such as opening grocery stores in these communities. It would be helpful for the authors to either propose potential policies or interventions that can address NSES or discuss existing interventions that have aimed to address this.

REVIEWER	Carla Blázquez-Fernández Universidad de Cantabria (SPAIN)
REVIEW RETURNED	22-Jan-2019

GENERAL COMMENTS	The study is useful but incomplete. Paper balanced in its different parts and well structured. A contribution that deserves to be considered for their publication, but not accepted in its actual state. The authors should justify using that 5 databases, that in fact, are the most commonly used (an additional one could be the Cochrane Library)…

	Another important fact is determining the exact period under analysis that should be also included in the research strategy.
	Besides, in that research strategy, and since the search formula is not the same for each database, it would be interesting to include textually what was entered in each database.
	In the same line, in the first square of the PRISMA diagram it should be indicated the number of records associated per database. That is, the desegregation of the 2,375 studies. At this regard, I am worry about the shortage of records obtained in the first search step.
	Authors should reinforce that explanations.

REVIEWER	Adrian Sayers University of Bristol
REVIEW RETURNED	23-Jan-2019

GENERAL COMMENTS	Review appears comprehensive.
	There is an omission in the search terms, in the uk, the area based index of deprivation is The index of multiple deprivation, im not sure if this explains the lack of Uk articles in the review.
	I think the search should be re-run including this term, to see if any additional articles would be found
	In the forest plots they need to more clearly indicate what the comparator is, i.e. is it normal weight or underweight?
	The authors should reflect more on the problems with interpreting area based indicies of deprivation, i.e. the potential for ecological fallacy and within and between decomposition of analyses in longitudinal studies.
	Opposed to RCT efficient studies that track an individuals location and change in location and obesity status are likely to be informative.
	It maybe that an obesogenic lifestyle is more of a trait than a state that is easily modified.
	Nice work.

VERSION 1 – AUTHOR RESPONSE

Reviewers' Comments to Author:
Reviewer: 1
Reviewer Name: Michelle Wong
Institution and Country: U.S. Department of Veterans Affairs
USA
Please state any competing interests or state 'None declared': None declared

Overview: In this paper, the authors synthesize through a meta-analysis the association between neighborhood socioeconomic status and overweight/obesity. The authors note that while there has been significant interest in research in this area, there has yet to be a comprehensive meta-analysis of these findings. Their study fills this gap, which has implications for addressing the obesity epidemic through broader policies beyond individual-level behavior change.

The authors conduct a comprehensive review of the literature, use methods that are appropriate for meta-analyses, and their conclusions are reasonable given the results.

Specific comments:

Introduction:
1. The authors switch between several terms: neighborhood socioeconomic status (NSES), neighborhood deprivation, and poor neighborhoods. It would be helpful if the authors chose one of these terms and used it consistently throughout the manuscript.
- R: We now have consistently used neighborhood socioeconomic status (NSES) throughout the document.
2. The introduction could be strengthened by more discussion about potential mechanisms linking NSES to obesity beyond just aspects of the built environment.
- R: Done as advised. Refer to page 4, lines 15-30.
3. Related to this, there are a variety of ways of conceptualizing NSES, beyond just poverty, and this is important for the proposed mechanisms linking NSES to obesity (e.g., resources, education, collective norms and collective efficacy, social norms). It is unclear how the authors are considering NSES.
- R: we agree with the reviewer that there are a variety of ways to measure NSES. Plus, it is worth noting that there is neither a uniform nor a standard approach in the literature with regard to measures of NSES. We, however, used the approach most commonly used in the existing literature, which is to measure NSES by composite index, rather than by just one parameter. Though the specific set of variables varied from one study to another, the most commonly used variables to develop NSES index include property ownership, the proportion of households owned by residents, the proportion of employed residents, the proportion of car ownership by residents, availability of health-promoting amenities, literacy rate, etc. in the neighborhood etc. This information was already included in the manuscript.
- The comment has been addressed by providing more statements about the measures of NSES in the manuscript. Refer to page 6, lines 3-17. Besides, we now have provided information on the challenges and limitations of NSES measures in the discussion section. Page 14, lines 12-17.
Methods
4. Page 7, line 9: what do the authors mean by "from inception"? What is the start date range for the search period?
- R: We couldn't state a specific start date the search because it was done for articles published until June 15, 2018. That is why we used the phrase 'we searched databases for articles published from inception to June 15, 2018'. In case the statement improves, we have now replaced it by 'we searched databases for articles published until June 15, 2018'. Page 5, line 19.
5. It would be helpful to know a bit more about what criteria the Newcastle-Ottawa Scale uses to evaluate the study quality
- R: Done. Please refer to page 9, lines 10-18.
Results
6. I am surprised by how few studies were included in the meta-analysis. My impression of the literature is that more studies have assess the relationship between NSES and obesity. Would be helpful to know more about the studies that were included and studies that were not included in the meta-analysis. For example, did the authors consider studies that looked at a variety of neighborhood

exposures, one of which is NSES? What about studies that came out of Moving to Opportunity (e.g., Chetty 2016 article that the authors cite)?

- R: The articles were few mainly because the outcome of interest in the previous version were only overweight and obesity. Studies that reported on NSES-BMI were not included, as the objective was to evaluate only link of NSES to overweight and obesity. We now have added NSES-BMI studies and the number of studies has doubled during the revision, from 10 to 20. The article by Chatty et al did not report on any of the measures of interest for this meta-analysis, no OR, RR or beta on NSES link to BMI. Thus, doesn't qualify for inclusion.

7. Results from the assessment of study quality is not included in the Results.

- R: Done now. Refer to Table 1. Page 9

8. The authors distinguish between whether the studies included in the meta-analysis used NSES or NDI in Table 1, but do not provide any information about how they are distinguishing between these two terms

- R: We used these terms as reported by the original studies themselves. However, due to the lack of a uniform and standardized approach for NSES measures, the terms seem interchangeable with a minor difference. We have looked at how the individual studies constructed the indices and learned there is no uniformity from study to study, though there are some commonalities. In this case, though the difference is not clear, to be safer we did subgroup analyses by NSESI (neighborhood socioeconomic index and neighborhood deprivation index). And, we did not find a significant difference in the NSES-Obesity association by the NSES indices. The comment is addressed by providing additional information on page 6, lines 3-17. Besides, we now have provided information on the challenges and limitations of NSES measures in the discussion section. Page 14, lines 12-17.

9. Related to this even within NSES and NDI, there is significant heterogeneity in how these indices are created (e.g., what factors go into the indices) and how they are measured and operationalized in the regression models (e.g., continuous measure, quartiles/quintiles/deciles, comparison of highest vs. lowest…). It would be helpful to have a sense of how the studies measuring NSES and NDI.

- R: Done (please refer to the response to the previous comment)

10. In general, it would be helpful to have more summary information about the studies. E.g., what characteristics did the studies control for?

- R: Done. Please refer to Table 1, page 9.

Discussion

11. The difference in the pooled findings between the cross-sectional and longitudinal studies is interesting and warrants further discussion, particularly the lack of association in the longitudinal studies.

- R: we now have added a half-page rationale on the discrepancy of NSES-obesity link by study design (Refer to page 12, line 12 to page 13, line 18).

12. It seems like a randomized controlled study to assess the effects of NSES on obesity would be challenging/nearly impossible, with both practical and ethical issues. How do the authors envision such a study? Or perhaps the authors could propose other study types that can assess causality that are stronger than the numerous extant cross-sectional studies?

- R: Comment well noted and the statement has now been modified to mean more of longitudinal studies. Page 13, lines 28-29.

13. The authors mention that obesity interventions have focused on providing information to address the individual-level determinants. However, there have been efforts to improve health promoting resources in low NSES communities e.g., built environment changes such as opening grocery stores in these communities. It would be helpful for the authors to either propose potential policies or interventions that can address NSES or discuss existing interventions that have aimed to address this.

- R: We have paraphrased the statement and provided some examples, including the need to avail health enhancing facilities closer and available to everyone as much as possible. Page page 14, lines 5-8

- The reviewer is much thanked for his/her valuable comments which indeed improved the manuscript. We also appreciated the time and effort the reviewer put on the work.

Reviewer: 2
Reviewer Name: Carla Blázquez-Fernández
Institution and Country: Universidad de Cantabria (SPAIN)
Please state any competing interests or state 'None declared': None declared.

The study is useful but incomplete. Paper balanced in its different parts and well structured. A contribution that deserves to be considered for their publication, but not accepted in its actual state.

The authors should justify using that 5 databases, that in fact, are the most commonly used (an additional one could be the Cochrane Library)…
- R: Done by checking Cochrane library but did not new articles.
Another important fact is determining the exact period under analysis that should be also included in the research strategy.
- R: Done by stating 'we searched databases for articles published until June 15, 2018'. Please refer to page 5, lines 18-19.
Besides, in that research strategy, and since the search formula is not the same for each database, it would be interesting to include textually what was entered in each database. In the same line, in the first square of the PRISMA diagram it should be indicated the number of records associated per database. That is, the desegregation of the 2,375 studies. At this regard, I am worry about the shortage of records obtained in the first search step. Authors should reinforce that explanations.
- R: Done and the PRISMA flow chart has been updated to reflect the records found from each database. Plus, as we have added an additional objective during the revision, NSES-BMI link, for which 10 articles found and included in the revision. Please refer to Figure 1 and Table 1 on page 9.
- The reviewer is much thanked for his/her valuable comments which indeed improved the manuscript. We also appreciated the time and effort the reviewer put on the work.

Reviewer: 3
Reviewer Name: Adrian Sayers
Institution and Country: University of Bristol, UK
Please state any competing interests or state 'None declared': No Competing Interests to Declare

Review appears comprehensive.

There is an omission in the search terms, in the uk, the area based index of deprivation is The index of multiple deprivation, im not sure if this explains the lack of Uk articles in the review. I think the search should be re-run including this term, to see if any additional articles would be found.
- R: We have checked it but could not find any more eligible articles.
In the forest plots they need to more clearly indicate what the comparator is, i.e. is it normal weight or underweight?
- R: Done by including information on the comparators for all outcome measures (overweight, obesity). Same has been done for the exposure comparator (NSES). The information has been incorporated into the title of the forest and funnel plots. Please refer to the title of the figures on page 19.
The authors should reflect more on the problems with interpreting area based indicies of deprivation, i.e. the potential for ecological fallacy and within and between decomposition of analyses in longitudinal studies. Opposed to RCT efficient studies that track an individuals location and change in location and obesity status are likely to be informative. It maybe that an obesogenic lifestyle is more of a trait than a state that is easily modified. Nice work.
- R: Done. Please refer to page 14, lines 11-31.

- The reviewer is much thanked for his/her valuable comments which indeed improved the manuscript. We also appreciated the time and effort the reviewer put on the work.

VERSION 2 – REVIEW

REVIEWER	Michelle Wong
	U.S. Department of Veterans Affairs, United States
REVIEW RETURNED	31-Jul-2019

GENERAL COMMENTS	Overall: The authors have done a good job of addressing many of the initial concerns. Abstract: 1. Page 3, line 15-16: Consider using the terms "low socioeconomic status" or "lower SES" instead of "poor" and "high SES" or "higher SES" instead of "better-off" neighborhoods. Introduction 2. Page 7, line 17 – 19: The authors need to link the "stress" hypothesis to neighborhoods. Include a phrase with more detail about the "stress hypothesis" i.e., lower SES neighborhoods expose residents to more psychosocial stressors. Methods 3. Did the authors consider differences in how the studies defined high vs. low SES neighborhoods? 4. Did the authors include studies that modeled NSES as a continuous variable or categorically (e.g., deciles)? If these studies were included, how were those estimates pooled with studies that dichotomized NSES? 5. Please provide a brief description of how the sensitivity analyses were conducted. Discussion 6. In the discussion of the discrepancy between the cross-sectional and longitudinal studies, one potential explanation is the potential for reverse causality in the cross-sectional analyses, and for this reason, longitudinal studies are generally preferred to look at the incidence of obesity or change in BMI over time. Authors should address this point in the discussion about the cross-sectional and longitudinal study discrepancies. 7. Page 23, lines 15- 17 to page 24, lines 6 – 8: The authors seem to suggest that the one of the main mechanisms linking neighborhood SES to obesity is through an obesogenic built environment. However, the research linking the built environment, particularly the food environment, to weight gain is highly mixed, including many studies that have yielded null findings. In fact, there may be an even stronger and more consistent relationship between NSES and obesity, suggesting that something beyond just an obesogenic built environment is contributing to weight gain.

REVIEWER	Carla Blázquez-Fernández
	Universidad de Cantabria (SPAIN)
REVIEW RETURNED	20-Aug-2019

GENERAL COMMENTS	The revised version has solved most of the problems raised in the previous version. I think the article can be accepted for publication.

REVIEWER	Adrian Sayers University of Bristol
REVIEW RETURNED	29-Jul-2019

GENERAL COMMENTS	The Literature search in the marked up copy doesnt agree with the supplementary document. I am little concerened that index of multiple deprivation and obesity didnt lead to any additional articles. a quick pubmed search using the following terms yeilded a number of results which seem relavent but not included. ("index of multiple deprivation" OR IMD) AND Obesity I think the authors need to carefully review the results again. As to my previous comments on ecological fallacy, i am not sure how these have been addressed on page 14, lines 11-31

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Michelle Wong

Institution and Country: U.S. Department of Veterans Affairs, United States

Please state any competing interests or state 'None declared': None declared

Overall: The authors have done a good job of addressing many of the initial concerns.

Abstract:

1. Page 3, line 15-16: Consider using the terms "low socioeconomic status" or "lower SES" instead of "poor" and "high SES" or "higher SES" instead of "better-off" neighborhoods.

-Response: Done throughout the manuscript by replacing 'poor neighborhood' by 'low SES neighborhood' and 'rich or better-off neighborhood' by 'high SES neighborhoods.

Introduction

2. Page 7, line 17 – 19: The authors need to link the "stress" hypothesis to neighborhoods. Include a phrase with more detail about the "stress hypothesis" i.e., lower SES neighborhoods expose residents to more psychosocial stressors.

-Response: Done. Refer to page 4, lines 27-32.

Methods

3. Did the authors consider differences in how the studies defined high vs. low SES neighborhoods?

-Response: Yes, it was considered. In some studies, NSES was categorized into tertiles groups (low, medium, high), in others, into quantiles. As there is no consistent or uniform measurement and categorization of NSES measures, we based our analysis based on the estimates which compared the highest and lowest NSES categories indicated in the study. Thus, when a study reported multiple NSES comparisons, we extracted the estimate in which the lowest and the highest categories was compared. This has been included in the manuscript (Page 7, lines 11-14).

-Besides, there was no uniformity in the specific set of variables used to construct NSES indexes. For this, we have conducted subgroup analyses by NSES measures and reported subgroup-specific estimates. Additionally, these issue of lack of uniformity in the measurement and classification of NSES has been already included as a limitation of the study. Refer to page 15, lines 28-33 (strength and limitations section).

4. Did the authors include studies that modeled NSES as a continuous variable or categorically (e.g., deciles)? If these studies were included, how were those estimates pooled with studies that dichotomized NSES?

-Response: We did not find articles in which NSES was treated as a continuous variable. For the second point, please check the response to comment number 3 above.

5. Please provide a brief description of how the sensitivity analyses were conducted.

-Response: Done. Refer to page 8, lines 17-23.

Discussion

6. In the discussion of the discrepancy between the cross-sectional and longitudinal studies, one potential explanation is the potential for reverse causality in the cross-sectional analyses, and for this reason, longitudinal studies are generally preferred to look at the incidence of obesity or change in BMI over time. Authors should address this point in the discussion about the cross-sectional and longitudinal study discrepancies.

- Response: Done. Refer to page 14, lines 26-32.

7. Page 23, lines 15- 17 to page 24, lines 6 – 8: The authors seem to suggest that the one of the main mechanisms linking neighborhood SES to obesity is through an obesogenic built environment. However, the research linking the built environment, particularly the food environment, to weight gain is highly mixed, including many studies that have yielded null findings. In fact, there may be an even stronger and more consistent relationship between NSES and obesity, suggesting that something beyond just an obesogenic built environment is contributing to weight gain.

- Response: The remark is well noted and our use of obesogenic environment is in its broader sense, encompassing various aspects of it which might increase the risk of obesity. These include the food environment, psychological situations of the area, crime and safety, lack of health-enhancing facilities like parks and exercise facilities, etc. It has been incorporated in the discussion section.

- Thank you for your valuable comments, time and effort put on the work.

Reviewer: 2

Reviewer Name: Carla Blázquez-Fernández

Institution and Country: Universidad de Cantabria (SPAIN)

Please state any competing interests or state 'None declared': None declared.

The revised version has solved most of the problems raised in the previous version. I think the article can be accepted for publication.

Reviewer: 3

Reviewer Name: Adrian Sayers

Institution and Country: University of Bristol, UK

Please state any competing interests or state 'None declared': None declared

The Literature search in the marked up copy doesn't agree with the supplementary document.

-Response: There were errors in the figures and have now been corrected.

I am a little concerned that index of multiple deprivation and obesity didn't lead to any additional articles. a quick PubMed search using the following terms yielded a number of results which seem relevant but not included. ("index of multiple deprivation" OR IMD) AND Obesity. I think the authors need to carefully review the results again.

-Response: The reason why we couldn't find additional articles after including 'Index of multiple deprivations' could partly be because there is no separate MeSH term for it and it is indexed under the other MeSH terms we used and which seems broader; such that the IMD based articles were already captured by the other terms (area deprivation, neighborhood economic condition, etc…). Besides, we included only the studies that fulfilled all the eligibility criteria. For example, we did not aim to include studies that used none-BMI weight measures, like weight change, waist circumference, studies that used BMI on centile scales, unhealthy population groups, etc. We also not aimed to include studies that focused on school, working place or household deprivation, rather than the residential neighborhood environment. Because of these reasons, the number of included studies

might be lower and this has been mentioned as a limitation in the manuscript. Besides, the article was submitted in 2018, with a literature search period 'until June 15, 2018' and would normally be expected studies after the search was done and manuscript submitted.

-To incorporate the comment, in this revision, we have updated the search period to 'until September 25, 2019'. This resulted in the inclusion of the following study done in UK. Changes in the summary estimates due to the inclusion of the study and re-analyses of the data have been updated throughout the manuscript, the tables, and the figures.

Walker IV, Cresswell JA. Multiple deprivation and other risk factors for maternal obesity in Portsmouth, UK. J Public Health (Oxf) 2019;41(2):278-86.

As to my previous comments on ecological fallacy, i am not sure how these have been addressed on page 14, lines 11-31

- Response: We share the comment. The included studies are mainly ecological in design. Thus, this study also shares the limitations of ecological study design. Besides, all the included studies are observational in design. Thus, it also shares the limitations of observational study design. Thus, the limitation section of the study has now been updated to reflect the above points as limitations. Please refer to page 16 lines 8-13.

- Thank you for your valuable comments, time and effort put on the work.

VERSION 3 – REVIEW

REVIEWER	Michelle Wong
	U.S. Department of Veterans Affairs, United States
REVIEW RETURNED	09-Oct-2019

| GENERAL COMMENTS | The authors have adequately revised and addressed all of concerns with the paper. |

REVIEWER	Adrian Sayers
	University of Bristol
REVIEW RETURNED	07-Oct-2019

| GENERAL COMMENTS | fine |