Author's response to reviews

Title: Trends in child and adolescent obesity prevalence according to socioeconomic position: protocol for a systematic review

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Author's response to reviews: see over
Dear Editors,

Thank you for the opportunity to revise our manuscript MS: 2111918965121944 Trends in child and adolescent obesity prevalence according to socioeconomic position: protocol for a systematic review. Thank you to both reviewers for the considered feedback on our review protocol. We have prepared a revised manuscript, where changes made are coloured in red, and below provide our responses to the reviewers’ comments.

We look forward to your response.

Kind regards,

Alexandra Chung
Paul Shekelle Comment 1

This is an important topic. I'm not sure a systematic review is the way to answer this question, particularly as what is being looked for is a recent change in trends. Given that it takes several years for original data to get into the published literature, and then another few years for a sufficient number of such studies to be published such that a systematic review can be meaningfully performed, doing a review now in 2014 would be looking at best at data from 2009, and more likely much earlier. But, nevertheless, the availability and currency of the data is one of the goals of a systematic review. So if the authors wish to go forward, it's justifiable to do so.

Thank you for the comment. We are confident that we have sufficient data for a systematic review of recent trends. Rokholm et al (2010) published a review of studies that reflected recent trends at the time, including seven articles from four countries that reported on data between 1999 and 2010 by socioeconomic position. We have identified additional relevant literature, some of which has been published since that time, allowing us to explore original data as recent as 2011. Further, our inclusion of a search of grey literature may identify additional recent data for the review.

Paul Shekelle Comment 2

In their inclusion criteria, they state that "only studies from developed countries will be considered". I think this would benefit from some specificity here. Do they plan on using some external criterion standard what is and what is not a developed country? If, so they should probably reference it here. If not, they probably want to state here what they consider the "developed" countries. England, Canada, the USA, etc are easy. But what about Greece? Poland? The areas that were formerly East Germany? Estonia? There are probably examples in southeast Asia that are similarly gray, so they will want to think this through. Differentiating Singapore from Nepal is going to be easy. But differentiating some other countries may not be so easy.

Thank you for this comment. We agree that definitions of developed countries are not always clear. Our list of included countries is based on the current list of members of the Organisation for Economic Co-operation and Development (OECD). We have amended the manuscript to provide this definition (p.7) and clarified this elsewhere in the manuscript (p.8 and Table 1).
“As a proxy for developed countries, we will focus the search on literature from countries that are members of the Organisation for Economic Co-operation and Development (OECD).” (p.7)

“Only studies from OECD member countries (chosen as a proxy for developed countries) will be included.” (p.8)

Paul Shekelle Comment 3

Also in the eligibility criteria: the authors state they will include studies with two or more time points. In the abstract they describe their search for “cross sectional studies ...with data recorded at two or more time points....". I think I know what they mean here, but the juxtaposition of "cross sectional" (which implies no time component) with "two or more time points" will confuse some readers. I think what the authors mean here are studies that examine annual or biannual cross sectional data across a number of years, such as some national population survey of weight that occurs every year or two. Since the same people aren't necessarily participants in each year of the survey, it's not really correct to call this a longitudinal cohort, but nevertheless calling it all cross sectional is likely to confuse readers unless there's a little more explanation. I don't know of a good term for this kind of study - "serial cross sectional studies...with two or more time points..."?

Thank you for alerting us to this potential confusion. We have made amendments to the abstract and methods in the manuscript (p.4 and p.7) to clarify the types of studies that will be included in the review.

“Articles that report on a series of cross sectional studies; describe one or more measure of obesity with data recorded at two or more time points since 1990; and report trends by at least one indicator of socioeconomic position will be included.” (p.4)

“Articles will be included if they report socioeconomic trends in the prevalence of obesity in children and/or adolescents aged 2-18 years from at least two time points since 1990.” (p.7)

Paul Shekelle Comment 4

The authors’ assessment of quality is certain to cause some concern, both in terms of the actual criteria and in terms of the aggregation into a score. The representativeness of the sample to some larger
population would seem to be paramount here, and I don’t see it anywhere in the list. Readers will not consider the eligibility criteria as sufficiently homogenous such that all included studies can be generalized to a larger population, which is also made apparent by the data extraction elements (schools, for example). Serial cross sectional studies of weight and socioeconomic position from classrooms at a school or schools in one part of one country have little basis for generalization to the total population of that country. So there needs to be some measure or assessment of how representative the intended population is (national, regional, local, etc.) in addition to the measure about the representativeness of participation (the rate to exceed 85% - and why was 85% chosen? I have often seen 70% or even 80% chosen as a threshold, this is the first paper I have seen that suggests 85% be chosen - readers will be curious where this came from). Next, the “sample size of at least 5000 participants” will cause readers to wonder - why 5000? Why not 4000, or 1,000, or 10,000? Can the authors provide some kind of rationale for this choice? I presume it is related to some measure of power, but if so they ought to be able to defend this choice. I am also surprised that in the quality assessment there is no criterion about how the key SEP variable is measured. They have a criterion for the difference between objective and self-report for the weight, but there must be some similar concern about how SEP is assessed, not just that it is assessed the same way over time. I suggest the authors look at both the QUADAS criteria and the criteria proposed by Hayden and colleagues in Annals of Internal Medicine 2013, since these topics (diagnosis and prognosis) share many similarities in terms of the risk of bias with the authors proposed assessment of the association between weight and SEP. And then of course the aggregation into a scale needs justification. If they are going to use a scale, then they will need to be able to reference that the threshold on the scale has some documented relationship with differences in study outcome, for examples of such in the literature see Moher 1998 in the Lancet or van Tulder in SPINE in 2009. The synthesis of these data are going to be tricky, as the authors will need to make lots of judgments about which studies have populations and measures of obesity and measures of SEP are sufficiently similar to be treated together, in a pooled result or even in a narrative result. There’s no recipe to follow here, so careful judgments and explanations are what readers are going to want to see.

Thank you for your detailed feedback and the references to the literature. We have used this to inform discussion amongst co-authors and further reading of the literature around quality assessment. As a result we have moved away from an aggregate scale and derived quality criteria by selecting relevant questions on internal and external validity from an existing source - the Effective Public Health Practice
Project’s quality assessment tool for quantitative studies. We have updated the manuscript to reflect this change (p.4 and p.9).

“Quality of included studies will be evaluated according to criteria that consider both internal and external validity.” (p.4)

“The quality of included studies will be evaluated independently by two authors, according to criteria adapted from an existing quality assessment tool for quantitative studies from the Effective Public Health Practice Project (EPPHP) [9]. The questions will assess internal and external validity with questions on selection bias, study design, confounders, data collection and data analysis. We will perform a sensitivity analysis to evaluate the potential effect of study quality on our conclusions by repeating our analysis on only those studies with high quality ratings for selection bias, confounding and data collection methods.” (p.9)

Carolyn Summerbell Comment 1
The authors seek to answer an extremely important and interesting, i.e. whether the seemingly levelling off of prevalence of childhood obesity in various countries of the world is the case across all socioeconomic strata. The authors talk about including evidence from ‘developed’ countries, but excluding evidence from ‘developing’ countries. Why? Surely one of the important things we want to know about is the trend in prevalence by SES in countries experiencing nutrition transition. There is no rationale for excluding ‘developing countries’.

Thank you for the comment. We have excluded developing countries from our review because we expect obesity trends to be quite different in these countries for two reasons. Firstly, because developed and developing countries are at a different stage in the nutrition transition, and secondly, because the association between socioeconomic position and obesity differs between developed and developing countries. We intend to discuss this in further detail in the article where we report the findings of our review.

Carolyn Summerbell Comment 2
From my limited knowledge of this topic, much of the sort of data which the authors appear interested in, is not published in peer reviewed journals. It is published in government and NGO type reports. For
me, if the authors are truly interested in the question they pose, it would seem limited to simply search the evidence published in peer reviewed journals. If the authors only looked at evidence in peer reviewed journals, they may well produce a biased result. I do think that this type of question is better answered by systematically contacting and analysing all countries governmental organisations and relevant NGOs – including WHO and IASO (as well as looking at the literature published in peer review journals) and potentially performing some secondary analysis on the data.

We agree that there is more than one possible strategy to identify socioeconomic trends in obesity. In this instance, we have chosen to conduct a systematic review of articles identified through a search of the peer reviewed literature, selected grey literature databases including the Virtual Library for Public Health and the System for Information on Grey Literature, and websites from relevant organisations. We believe that it is critical to identify as soon as possible whether apparent improvements in childhood obesity are being experienced equally across socio-economic groups. The proposed method is the quickest way to further our understanding of this. We agree that if our hypothesis is verified and trends do not appear to be the same across groups, this would pave the way for a more extensive study of empirical data.