

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	THE INFLUENCE OF THE FOOD ENVIRONMENT ON OVERWEIGHT AND OBESITY IN YOUNG CHILDREN: A SYSTEMATIC REVIEW.
<b>AUTHORS</b>	Dick, Smita ; Osei-Assibey, George; Macdiarmid, Jennie; Semple, Sean; Reilly, John; Ellaway, Anne; Cowie, Hilary; McNeill, Geraldine

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Jonathan R Treadwell PhD ECRI Institute, USA. No competing interests.
<b>REVIEW RETURNED</b>	21-Jun-2012

<b>GENERAL COMMENTS</b>	<p>The article was apparently submitted by the authors on 5/22/12, and I received the invitation to review it on 6/20/12. I suppose it took a month for BMJ Open to decide whether to send it for peer review.</p> <p>Your overall conclusion is that you have “shown that the micro and macro environments of children are important in preventing the rise in obesity”. This is stated very strongly. From this, one would infer that the evidence leaves no doubt whatsoever. When I read that, I thought WOW there must have been several long-term large multicenter blinded randomized trials, in order to support such certainty. When I went to the section called “Characteristics of included studies”, to see what the study designs were, there was no text to help me. Instead it just mentioned “intervention studies” and “short-term experiments”. It wasn’t clear whether the word experiment was being used instead of randomized. Or whether “longitudinal” was being used instead of “case series”. So I couldn’t tell about randomization or multicenter or blinding or attrition or any of the usual examined when considering the risk of bias in research. So I traipsed to the appendix. In the appendix I could also not tell whether studies were randomized, or even the more fundamental issue of what interventions were being compared by each study. I gave up in frustration.</p> <p>Apparently you did not even rate the risk of bias. This is a fundamental tenet of a good systematic review. It is not sufficient to have a few sentences in the discussion about “limitations”. Your Methods section should contain a section on how you assessed the risk of bias of each datapoint of each study. Yes, I say each datapoint, because within a study there can be more bias for some outcomes than others (for example the unblinded outcomes), and also there can be more potential bias at some timepoints than others</p>
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	<p>(for example, the longer timepoints have greater attrition and therefore greater potential for bias because maybe the groups got unbalanced over time). Your results section should have a subsection devoted to risk of bias. Your appendix should have a table of each studies risk of bias ratings.</p> <p>Nowadays, many systematic reviewers also rate the <i>strength of evidence</i> for each outcome of each comparison of interventions. This rating (High, Moderate, Low, or Insufficient) considers not only the risk of bias, but also the consistency, precision, directness, how big the effect is, and other factors. See the paper by Owens et al. in JCE in 2009. You should say why you are not rating the strength of evidence, given that so many reviews do so.</p>
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<b>REVIEWER</b>	Dr Ilona Koupil Professor of Health Equity Studies Stockholm University/Karolinska Institutet Sweden
<b>REVIEW RETURNED</b>	16-Jul-2012

<b>THE STUDY</b>	This is a literature review, not an analysis of original data.
<b>GENERAL COMMENTS</b>	<p>This is an original and timely review of environmental influences on risk of childhood overweight and obesity. The review is carefully designed and well performed.</p> <p>The main concern is that the reviewed literature is very heterogeneous in terms of type of intervention studies, outcome measure, study size or length of follow-up. This makes it difficult to provide a more concise summary of the review.</p> <p>I have some relatively minor comments for the authors to consider when preparing the final version of their manuscript:</p> <ul style="list-style-type: none"> <li>- it would be useful to comment on and clarify the length of follow-up in the respective studies... have long term outcomes been evaluated?</li> <li>- sometimes it is not clear whether the study populations included children who were underweight or overweight/obese at the start of the intervention/follow-up. It would be useful to comment on the actual distribution of BMI in the study populations, especially, since the outcomes are also important to be evaluated in the context of eating disorders, underweight, etc.</li> <li>- the quality of the reviewed studies does not seem to be systematically evaluated or reported. Would it be possible to add some more discussion on that?</li> <li>- the heterogeneity of the study designs and outcomes makes it very difficult to summarize the outcomes - would it be at all possible to do some semi-quantitative summary or the effect sizes?</li> </ul>

<b>REVIEWER</b>	Prof. Orly Manor Braun School of Public Health & Community Medicine Hebrew University-Hadassah
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	Jerusalem, Israel
<b>REVIEW RETURNED</b>	20-Jul-2012

<b>GENERAL COMMENTS</b>	<p>The paper aims at reviewing the evidence regarding the influence of food environment on overweight and obesity among children up to 8 years old. Inclusion criteria included intervention or longitudinal studies, studies related to diet that focused on prevention. Search in electronic data bases and other sources yielded 35 studies that met the inclusion criteria. At a preliminary stage 9 areas for dietary determinates were identified and studies were categorized into these areas.</p> <p>Main comments:</p> <p>The topic of the paper is of interest and the authors conducted a thorough search and a fine classification of the studies. However, the integration of the evidence is not without flaws, due to substantial heterogeneity in designs, focuses and outcomes of the various studies. Furthermore, conclusions regarding the evidence on a long-term effect of the various exposures /interventions- are not completely clear.</p> <p>Indeed, the authors admit that they couldn't perform a meta-analysis because of heterogeneity, yet this leads to a report which is only descriptive without any analytic component.</p> <p>The authors could reduce the homogeneity- for example by separating the experimental and observational studies.</p> <p>Another source of heterogeneity stems from various outcomes considered that is anthropometric as well as intermediary behavior. Again, these outcomes could be separated.</p> <p>Table 2, which summarize the results of the different studies lack clear information on the main outcome variable and more importantly on effect sizes.</p> <p>Additional comments:</p>
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	<p>In the abstract – lines 34-36 the authors mention activity habits. However physical activity was not the topic of the review.</p> <p>Page 9- top. Is the conclusion restricted to boys? Actually some of the studies were conducted in girls. See also lines 8-10 on this page. Is this reflected in Table 2?</p> <p>Page 9- Was large portion size associated with overweight? As suggested in the discussion. From the Table it seems that large portion size was only associated with food intake.</p>
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### VERSION 1 – AUTHOR RESPONSE

Reviewer: Jonathan R Treadwell PhD, ECRI Institute, USA. No competing interests. The article was apparently submitted by the authors on 5/22/12, and I received the invitation to review it on 6/20/12. I suppose it took a month for BMJ Open to decide whether to send it for peer review. Your overall conclusion is that you have “shown that the micro and macro environments of children are important in preventing the rise in obesity”. This is stated very strongly. From this, one would infer that the evidence leaves no doubt whatsoever. Conclusions have now been toned down.

When I read that, I thought WOW there must have been several long-term large multicenter blinded randomized trials, in order to support such certainty. When I went to the section called “Characteristics of included studies”, to see what the study designs were, there was no text to help me. Instead it just mentioned “intervention studies” and “short-term experiments”. It wasn’t clear whether the word experiment was being used instead of randomized. Or whether “longitudinal” was being used instead of “case series”. So I couldn’t tell about randomization or multicenter or blinding or attrition or any of the usual examined when considering the risk of bias in research. So I traipsed to the appendix. In the appendix I could also not tell whether studies were randomized, or even the more fundamental issue of what interventions were being compared by each study. I gave up in frustration. The new table now separates the cohort studies from the intervention studies with details of the type of intervention used e.g. RCT’s, Non RCT’s.

Apparently you did not even rate the risk of bias. This is a fundamental tenet of a good systematic review. It is not sufficient to have a few sentences in the discussion about “limitations”. Your Methods section should contain a section on how you assessed the risk of bias of each datapoint of each study. Yes, I say each datapoint, because within a study there can be more bias for some outcomes than others (for example the unblinded outcomes), and also there can be more potential bias at some timepoints than others (for example, the longer timepoints have greater attrition and therefore greater potential for bias because maybe the groups got unbalanced over time). Your results section should have a subsection devoted to risk of bias. Your appendix should have a table of each studies risk of bias ratings.

A new section on risk of bias has now been added to the ‘Results’ section-further details see reply to the overall comments from the authors.

Nowadays, many systematic reviewers also rate the strength of evidence for each outcome of each comparison of interventions. This rating (High, Moderate, Low, or Insufficient) considers not only the risk of bias, but also the consistency, precision, directness, how big the effect is, and other factors. See the paper by Owens et al. in JCE in 2009. You should say why you are not rating the strength of

evidence, given that so many reviews do so.

A ranking exercise was undertaken to assess the strength of evidence-see details in the results subsection on evidence rating and on risk of bias.

Reviewer: Dr Ilona Koupil  
Professor of Health Equity Studies  
Stockholm University/Karolinska Institutet  
Sweden

This is an original and timely review of environmental influences on risk of childhood overweight and obesity. The review is carefully designed and well performed.

The main concern is that the reviewed literature is very heterogeneous in terms of type of intervention studies, outcome measure, study size or length of follow-up. This makes it difficult to provide a more concise summary of the review.

I have some relatively minor comments for the authors to consider when preparing the final version of their manuscript:

- it would be useful to comment on and clarify the length of follow-up in the respective studies... have long term outcomes been evaluated?

Where available duration of the study has been added to the tables.

- sometimes it is not clear whether the study populations included children who were underweight or overweight/obese at the start of the intervention/follow-up. It would be useful to comment on the actual distribution of BMI in the study populations, especially, since the outcomes are also important to be evaluated in the context of eating disorders, underweight, etc.

See methods section.

- the quality of the reviewed studies does not seem to be systematically evaluated or reported. Would it be possible to add some more discussion on that?

- the heterogeneity of the study designs and outcomes makes it very difficult to summarize the outcomes - would it be at all possible to do some semi-quantitative summary or the effect sizes?

See reply to the overall authors comments above.

Reviewer: Prof. Orly Manor  
Braun School of Public Health  
& Community Medicine  
Hebrew University-Hadassah  
Jerusalem, Israel

The paper aims at reviewing the evidence regarding the influence of food environment on overweight and obesity among children up to 8 years old. Inclusion criteria included intervention or longitudinal studies, studies related to diet that focused on prevention. Search in electronic data bases and other sources yielded 35 studies that met the inclusion criteria. At a preliminary stage 9 areas for dietary determinates were identified and studies were categorized into these areas.

Main comments:

The topic of the paper is of interest and the authors conducted a thorough search and a fine

classification of the studies. However, the integration of the evidence is not without flaws, due to substantial heterogeneity in designs, focuses and outcomes of the various studies. Furthermore, conclusions regarding the evidence on a long-term effect of the various exposures /interventions- are not completely clear.

Indeed, the authors admit that they couldn't perform a meta-analysis because of heterogeneity, yet this leads to a report which is only descriptive without any analytic component.

The authors could reduce the homogeneity- for example by separating the experimental and observational studies.

This has now been done see the new table.

Another source of heterogeneity stems from various outcomes considered that is anthropometric as well as intermediary behavior. Again, these outcomes could be separated.

Table 2, which summarize the results of the different studies lack clear information on the main outcome variable and more importantly on effect sizes.

Main outcome variables are now clearly stated within the tables.

Additional comments:

In the abstract – lines 34-36 the authors mention activity habits. However physical activity was not the topic of the review.

This has been addressed see revised abstract.

Page 9- top. Is the conclusion restricted to boys? Actually some of the studies were conducted in girls. See also lines 8-10 on this page. Is this reflected in Table 2?

The sentence has now been restructured to make this clear.

Page 9- Was large portion size associated with overweight? As suggested in the discussion. From the Table it seems that large portion size was only associated with food intake.

This has now been corrected in the discussion section.

#### VERSION 2 – REVIEW

<b>REVIEWER</b>	Prof. Orly Manor Braun School of Public Health & Community Medicine Hebrew University-Hadassah Jerusalem, Israel
<b>REVIEW RETURNED</b>	09-Nov-2012

<b>GENERAL COMMENTS</b>	In the revised version and in the Author's Response, the authors addressed the issues raised by the three reviewers. The manuscript has improved substantially and I now find it suitable for publication in BMJ Open.
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## Correction

Osei-Assibey G, Dick S, Macdiarmid J, *et al.* The influence of the food environment on overweight and obesity in young children: a systematic review. *BMJ Open* 2012;**2**:e001538.

On page 9 under the section 'Outcome variables' the first sentence should read: 'Although within majority of the included studies outcome measures were BMI, BMI z-scores or changes in weight,<sup>19-22 35-44 47 49 50 52 53</sup> other studies only reported the outcomes as changes in energy intakes, gain in health-related and nutrition-related knowledge and taste preference scores.<sup>25 28-30</sup>

In tables 1 and 2 the following should have been inserted above the line

'Restaurants, fast food outlets and coffee bars; No studies':

'Food availability and access

No studies'

In the Acknowledgements section the acronym '(EDPHiS)' should have followed 'Environmental Determinants of Public Health in Scotland.'

*BMJ Open* 2012;**2**:e001538corr1. doi:10.1136/bmjopen-2012-001538corr1