**Author’s response to reviews**

**Title:** The World Health Organization's Health Promoting Schools framework: a Cochrane systematic review and meta-analysis

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**Author’s response to reviews:** see over
Dear Editor,

We are pleased to submit to you a revised version of our manuscript entitled “The World Health Organization's Health Promoting Schools framework: a Cochrane systematic review and meta-analysis” (MS: 2881694661310693).

We are very grateful for the reviewers’ comments and have revised the text in line with their helpful suggestions. These changes are documented in the table below.

We look forward to receiving your response.

Yours faithfully,

Rebecca Langford (on behalf of the co-authors)
Response to reviewers’ comments

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<td>This manuscript is an interesting one from position of the repeat of a similar study conducted in 1999. However, what was disappointing to this reviewer was that with 15 more years of studies that were conducted on this framework, the conclusions are the same. It was also disappointing to note that issues raised 15 years ago have not been addressed by researchers in this area. There appearances to have been no improvement in methods or types of methods for data collection with this 15 year period. One might expect to have this discussed in the discussion section.</td>
<td>We thank the reviewer for this comment and agree that this is a point worth emphasising in the discussion. We now include the following sentences: “The majority of studies did not provide data on long-term follow-up or economic costs so the sustainability and cost-effectiveness of this approach is largely unknown. Studies were also often underpowered and relied heavily on self-reported data. It is disappointing to note that many of these methodological issues were identified in a previous review of the HPS framework and little improvement has been made in the past fifteen years[13].”</td>
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<td>On page 12, the authors after presenting several pages of outcomes stating no evidence, concluded “our analysis showed positive intervention effects”. The positive results indicate a reduction in BMI that no reduction in the Z BMI. This is an area that should be enlightened in the manuscript.</td>
<td>We have now rephrased this section to clarify our findings, as follows: “HPS interventions focussing on physical activity alone found an average reduction in BMI of 0.38kg/m² but no effect was found for Physical Activity + Nutrition interventions. We found little evidence of effect for zBMI, other than in the single Physical Activity intervention [69].”</td>
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<td>Is the addition of 3 min of moderate-to-vigorous activity per day clinically important either to general physical health or obesity?</td>
<td>We agree that a three minute increase is a modest increase and have now rephrased the discussion to reflect this. “These effect sizes are small but are comparable to findings from other school based interventions[85-87]. Small effects scaled up to population level can produce public health benefits[88], although at present these potential gains appear modest.”</td>
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The focus of this review will be on the conclusion presented in the discussion section and the overly positive terminology used... While the next statement indicates a lack of effectiveness, there was weak evidence for six areas, no evidence for seven areas and lack of data to be able to determine outcomes in five areas. It is difficult for this reviewer to then draw to their conclusion on page 15 “our review demonstrates the potential benefits of this approach for health”. It would seem appropriate to apply the same evidence based standards as is applied to other preventative activities. If the standard of the US Preventative Task force for evidence is applied, the same conclusion might not be reached.

We thank the reviewer for highlighting the overly positive terminology. As a result, we have edited various sections of the manuscript to better reflect the evidence presented, as follows:

Abstract:
This Cochrane review has found the WHO HPS framework is effective at improving some aspects of student health. The effects are small but potentially important at a population level.

Discussion:
Our analyses showed modest positive intervention effects on average in reducing BMI, smoking and incidence of being bullied, and increasing physical activity, fitness, and fruit and vegetable intake.

HPS interventions focussing on physical activity alone found an average reduction in BMI of 0.38kg/m² but no effect was found for Physical Activity + Nutrition interventions. We found little evidence of effect for zBMI, other than in the single Physical Activity intervention[69].

These effect sizes are small but are comparable to findings from other school based interventions[85-87]. Small effects scaled up to population level can produce public health benefits[88], although at present these potential gains appear modest at best. Stronger evidence is available for HPS effects on smoking and bullying.

Our review demonstrates the potential benefits of this approach for some health outcomes but not others.

The authors are in a unique position to clearly
identify lessons learned over the past 15 years and not just the need to obtain educational outcomes data.

paucity of education outcomes. In response to the reviewer’s comment, we have added brief comments on other important conclusions from our review, as follows:

“Whilst our review has found evidence in favour of the HPS approach for a number of outcomes, it has also identified gaps in our knowledge base. We lack sufficient data at present to determine the effect of this approach for a number of health outcomes, particularly mental health and sexual health. We also identified an imbalance between which health topics were focused on at different ages. Physical activity and/or nutrition interventions tended to focus on younger children (<12 years) while substance use, violence, sexual health and mental health interventions usually targeted older students (12-14 year-olds). This imbalance is unjustified. Obesity does not just affect younger children[96]; we need to develop effective obesity-prevention interventions for older children too. Equally, two of the most effective Multiple Risk Behaviour interventions[21, 32] were conducted in elementary-school children, suggesting that intervening early may help prevent risk-taking in teenage years. We also identified the family/community domain to be the weakest aspect of the implementation of the HPS framework with most studies employing very minimal efforts to engage families (e.g. newsletter articles or flyers).

The majority of studies did not provide data on long-term follow-up or economic costs so the sustainability and cost-effectiveness of this approach is largely unknown. Studies were also often underpowered and relied heavily on self-reported data.”

I was not able to locate in the tables a list of the 67 trials. This would be useful for the reader of knowledge.

We agree that this would be useful. We have therefore changed Table 1 to give study design characteristics for each individual study, rather than summarising by intervention type.

Also, are more sensitive note, in the limitations, no acknowledgement was made of the authors competing interests and relationship the world health organisation. While this is a strength is also a limitation that should be acknowledged up front as it could influence the use of descriptive terminology.

We included a section on competing interests in the paper. We suspect that this paragraph was not included in the manuscript version sent to the reviewer. The paragraph reads as follows:

“KK was an investigator in three studies[29, 38, 39] included in the review and received royalties for the sale of these prevention curricula. She was not involved in the data extraction or interpretation of data from these studies for this review. EW has previously received funding to her institution and to herself from the World Health Organization for
unrelated pieces of work. RL has undertaken consultancy work for the WHO as part of a Delphi exercise into mental health and psychosocial support in humanitarian settings. The WHO had no role in the study design, data extraction, data analysis, data interpretation, or writing of the report. RC is a Director of a not-for-profit company, DECIPHer IMPACT Ltd, set up to enable organisations to use the DECIPHer ASSIST smoking prevention programme – a peer-led intervention for use with adolescents in secondary schools. She received fees paid into a grant account held by the University of Bristol and used to support further research activities. All remaining authors declare that they have no competing interests.”

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<td>This review/ metaanalysis of the WHO Health Promoting Schools framework is very well written, logically and consistently presented across a range of health and educational outcomes. I am a mere epidemiologist and not an expert in Cochrane systematic reviews but to my eye the manuscript adheres closely to that method. The research question is clear. Methods are appropriate and well described. Limitations, particularly the paucity of data for many outcomes, is acknowledged. The authors do identify that their manuscript is based on a Cochrane review which they conducted but I think this acknowledgement should be placed in the abstract. I have one discretionary revision suggest: The HPS intervention framework is based on 3 primary elements: 1. school curriculum, 2. Environment of the school, and 3. engagement of family and/or community. It would interesting and useful to compare the findings of this review to other</td>
<td>We acknowledge in the title that is paper is based on a Cochrane systematic review and meta-analysis. In the conclusion, we now mention again that this is a Cochrane review: “This Cochrane review found the WHO HPS framework is effective at improving some aspects of student health and shows evidence of promise in improving others. The effects are small but important at a population level.” We agree with the reviewer that this would be useful information. However, the heterogeneity of interventions included in other school-based reviews (which may include curriculum-only as well as more comprehensive approaches) mean these comparisons difficult to make. We now highlight this in the discussion and suggest the use of factorial designs to identify the relative importance of the different HPS domains. The new text</td>
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reviews (whether Cochrane or not) which have examined components of the framework. So a number of reviews exist of school curricular interventions. Given the limited number of HPS interventions for certain outcomes, it would be helpful to know how these outcomes fared in other reviews. Likewise, for those outcomes which had a goodly number of studies to review, does the HPS framework appear to be superior to interventions which included school curricula but without environmental and family/community components.

reads as follows:

“A further limitation of the review is that we are unable to compare the effectiveness of the HPS approach to simpler, less holistic interventions because most studies compared HPS interventions against no intervention or the school’s usual practice. While HPS effect sizes are broadly similar to results from other school-based systematic reviews [85-87, 96, 97], the latter often include different types of interventions ranging from ‘curriculum only’ interventions to more comprehensive programmes making meaningful comparisons difficult. Future studies should consider use of factorial designs to identify the relative importance of the three HPS domains and the way in which they interact.”

One other small point: in the abstract I was initially a bit confused by the difference in impact of between BMI and standardized BMI. Please clarify that.

We have now clarified this, as follows:

“On average across studies, we found little evidence of effectiveness for zBMI (BMI, standardized for age and gender), and no evidence for fat intake, alcohol use, drug use, mental health, violence and bullying others.”