Reviewer's report

Title: Systematic review of the effectiveness of antimicrobial rinse-free hand sanitizers for prevention of illness-related absenteeism in elementary school children

Version: 2 Date: 16 February 2004

Reviewer: Helen Worthington

Reviewer's report:

General
This systematic review has in general been well conducted however I have a few comments especially relating to the statistical analysis. I feel that although the meta analysis of this type of data is not well developed the authors should discuss the issues listed below. This will not change the conclusions of the review so I have simply included these statistical comments for consideration under the Minor Essential Revisions heading.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

1. I do not have much experience in the meta analysis of rates however analysing count data as rates is not always the most appropriate approach and is uncommon in practice (Handbook, Cochrane Library issue 1, 2004). This is because: (1) the assumption of a constant underlying risk may not be suitable; and (2) statistical methods are not as well developed as they are for other types of data. Although the authors do not undertake a meta analysis they indicate that this is the proposed method and present the results for individual studies as absenteeism rate per 100 student days. I am unsure whether the authors can assume constant underlying risk. Have the authors considered other methods for analysis data of this type? I feel that even if the authors do not change the analysis they have conducted they should justify why this approach has been selected.

2. Why was only data from the first period of cross-over studies extracted? This is not a recommended approach to the meta analysis of cross-over trials (Elbourne DR et al: Meta-analyses involving cross-over trials. International Journal of Epidemiology 31:140-149, 2002). The data from this trial needs reconsidering.

3. The data analysis section does not mention the analysis of cluster trials and I wonder if the analysis did take into account the clustering of children within schools in these trials. Frequently the results published in trial reports do not take the clustering into account correctly.

4. The search strategy was generally well conducted however I was concerned about the statement 'literature indices that the exclusion of trials published in non-English languages generally has little effect on summary treatment effect estimates'. The reference by Juni 2002 quoted does state that this is difficult to predict for individual reviews and the authors conclude that comprehensive literature searches are required independent of language of publication. The authors should reconsider looking at papers in non-English languages- as some of these will be published as
English abstracts in MEDLINE the authors should state how many were identified as eligible and consider getting any relevant papers translated.

5. I am unclear about whether any hand searching was conducted. The authors talk about ‘articles identified by hand search’ however it is unclear what journals have been hand searched and over what period.

6. Kappa statistic should be used to assess agreement as well as stating the percent agreement. This should be done for each quality item separately rather than an overall percent for all quality items. The statement, ‘Our overall agreement was 92.0 percent for quality assessment’, is insufficient.

Discretionary Revisions (which the author can choose to ignore)

What next?: Accept after minor essential revisions

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

None