Reviewer's report

Title: Hyperopia: a meta-analysis of prevalence and a review of associated factors among school-aged children

Version: 4
Date: 19 August 2014
Reviewer: Brendan Barrett

Reviewer's report:

The authors have combined Tables 1 & 2, conducted a meta-analysis and included a new figure 2. The paper is improved but still has some major issues.

Required Change: Discussion of how myopia onset and prevalence is relevant to the hyperopia story

The authors use the example of tallness and shortness in physical stature to make the case that hyperopia prevalence data alone have value without reference to myopia prevalence in the same population. Specifically they say “As a parallel it can be stated that populations which on average are tall have lower prevalence of short individuals, nevertheless one of these events does not case the other, they are complimentary and what needs to be examined are the causes”.

Although I recognise the point they are making I can’t agree! As the authors know well, as axial length (AL) increases with age, hyperopia diminishes; and as AL continues to increase, myopia develops. Thus, while hyperopia doesn’t cause myopia or myopia doesn’t cause hyperopia, both are caused by axial length anomalies (too big, myopia; too short, hyperopia), just like how limb length dictates height. Given that spherical refractive error hinges on axial length, and that emmetropization is complete by the age of 5 (the youngest children in this study), the variation with age in the prevalence of hyperopia (one of the main aspects of this paper) is only part of the story; myopia onset and progression is the other part because the axial length scale is continuous not dichotomous.

MAJOR COMPULSORY REVISION: Because emmetropization is over by the age of 5, the reduction in hyperopia prevalence is caused by increased axial length and, for many children (especially in Asia), the drift to myopia. However, myopia is not discussed at all. While the authors have stated (in their response to my comments) their reasons for not reporting myopia prevalence data here, they should at the very least include a paragraph in the discussion to acknowledge that the reduction is prevalence of hyperopia in not due to emmetropization (it’s too late for that), and so it must be due to the same process that leads to the onset and development of myopia in children and teenagers. As well as acknowledging this point, this paragraph could speculate that populations with more myopia or with earlier onset myopia might be expected to be the populations that show the biggest reductions in the prevalence of hyperopia, or the earliest reduction in hyperopia or both. Indeed, the authors may actually wish
to examine the evidence for this in the literature they examined, but I am not insisting on this [i.e. DISCRETIONARY REVISON]

Specific comments requiring consideration:
Abstract, method: PRISMA guidelines. [MINOR ESSENTIAL REVSION]
Abstract, method: “…through cycloplegic autorefraction or cycloplegic retinoscopy.” [i.e. add cycloplegic before retinoscopy, if it was the case that all retinoscopy studies included were also conducted under cycloplegia]. [MINOR ESSENTIAL REVSION]

Abstract, results: Figure 1 says 40 studies were included but here it says 41. Also (in next sentence) what is “summary effect”. Is this overall prevalence? [MINOR ESSENTIAL REVSIONS]

Abstract, conclusion: I don’t understand how the conclusions in the abstract relate to the material presented in the paper. For example, while the studies considered in the review may have used different criteria to classify hyperopia, were there many/any that had an insufficient sample size? In short, I’m struggling to understand how the conclusions presented in the abstract relate to the main manuscript. The same applies to the conclusion in the main text. Related to my main point about myopia: while ethnicity, and outdoor activities appear to affect prevalence of hyperopia, as does the definition of hyperopia (hence he need for consistent definition and agreed means for measurement), a large part of the variation must also stem from the underlying prevalence and severity of myopia in each population that is studied. [MAJOR COMPULSORY REVSION]

Background:
Although short, some of the background section seems to me to be irrelevant, and some important aspects of the hyperopia story are lacking or completely absent. Why give prominence to emmetropization when this is a paper about hyperopia in 5-18 year olds? How is a discussion of axial length and corneal curvature (their contribution to spherical equivalent refractive error) relevant here? I would have thought that more relevant topics for the ‘background’ and ‘discussion’ sections might be: how much hyperopia is a problem given that children have lots of accommodation and so can overcome it to see clearly? The issue with hyperopia is that we don’t really know how much of a problem it is because (i) we don’t know how much exists (this is one of the positive contributions of this paper) and (ii) even if we did know precisely how prevalent it is (at different severities), we still don’t have agreement about how it should be managed. As the authors point out in the conclusions, many think that low to moderate hyperopia can be left uncorrected, whereas others want to intervene to correct hyperopia even at modest levels. Isn’t this aspect of the hyperopia story deserving of greater attention? Isn’t his of interest to the reader in a paper about hyperopia? . [DISCRETIONARY REVSION]

Methods:
Some of the terminology needs attention. For example, in the meta-analysis
section the authors refer to (a) moderate hyperopia prevalence whereas I’m convinced they mean something different, specifically prevalence of moderate hyperopia ( (a) and (b) have completely different meanings despite the similar terminology!). This is more of an English language issue. [MINOR ESSENTIAL REVISION]

Results:
In the ‘hyperopia prevalence by age’ section, the authors mention that while most studies used a criterion of +2.00D of above, many did not. Is it valid to include prevalence studies that compare studies with different definitions for hyperopia? Maybe this was taken care of in the meta analysis? Was it? Please elaborate on this and, if not, justify how it valid to compare prevalence when the cut-off for defining hyperopia differed from +2DS or above?
The authors still don’t seem to define what they mean by ‘outlier’. Why, specifically, was Fotouhi’s study excluded? OK, the prevalence estimates were different: but how different did estimates have to be in order to be excluded?
[BOTH ARE MINOR ESSENTIAL REVISIONS]

Discussion:
1st sentence mentions “...several studies on the prevalence of hyperopia in childhood....”. Please add these citations. Also, precisely where do the authors specifically compare and discuss their meta-analysis results with the studies that showed a substantially higher or lower hyperopia prevalence, and explain (or try to) the likely reasons for these differences? Can it be added? If this doesn’t exist (and I couldn’t see it) please add it. [MINOR ESSENTIAL REVISIONS].

Figures:
Figure 2. Given that many readers will not be familiar with forest plots the figure legend should provide instructions as to how these plots should be interpreted. For example, it is not obvious that “effect summary” in fact means “prevalence”! The number in brackets after the prevalence (at each age) presumably represent the 95% confidence intervals? This should be clearly explained; even if this appears in the main text, it should also appear in the legend for clarity and to aid the reader understand the plots. Also, ‘anos’ appears instead of ‘years’. [MINOR ESSENTIAL REVISIONS]

General point:
There continue to be many typographical errors in the manuscript [e.g. in figure legends ‘metha’ analysis and ‘fostes’ plots; in the conclusion, ‘youger’] . Please ask a native English-speaker to fully proof read the manuscript. [MINOR ESSENTIAL REVISIONS]