Reviewer's report

Title: Convergence Insufficiency and Accommodative Insufficiency in Children

Version: 1 Date: 03 Jan 2019

Reviewer: Anna Horwood

Reviewer's report:

The authors have partly answered some of my concerns outlined in my review, but do not appear to have changed anything significant in the paper itself. There is a significant difference in opinion between what constitutes CI in paediatric ophthalmology practice and that in optometry, and a paper for this Journal's readership should reflect that. Even if the authors use the optometric definitions, they should at least acknowledge that many paediatric ophthalmologists or orthoptists would not recognise many of these children as "patients", especially if they were asymptomatic.

The paper's main findings are that the prevalence of "definite" CI (using the widely used CITT criteria - near exophoria, remote NPC and reduced fusion range - whether I agree with them or not) is 2.1%, but this is not even mentioned in the abstract, which implies that the prevalence of CI was 14%. This is highly misleading. It will have included "suspect CI's" with extremely mild problems, who may or may not have the condition. I think this study was sufficiently thorough to enable the authors to tell whether CI was present or not, so I am not sure what a "suspect" categorisation would trigger (further investigation / observation / treatment?). I do not agree that a small near exophoria constitutes a problem if convergence is good, so I contest the "low suspect" classification, and I would even argue that many people in the "high suspect group" do not necessarily have a problem either. I would prefer it if the authors prioritised reporting the 2.1% prevalence of definite CI in the text and abstract, and then just report their other findings for the different measures. They could comment that the CITT group have described them as "high or low suspect".

The authors have partially explained to me how the testing was carried out and standardised (but have not changed the text to inform the readers). They have not told us whether the participants were asked to try as hard as they could to stop it going blurred or double if they started to struggle. I can usually double a fusion range tested by a student just by changing my approach and instructions, so this is an important point to report and keep consistent.

The authors quote the literature on the high rate of co-morbidity of poor performance and CI/AI, with the clear implication that CI is causal to the poor performance (especially in the Discussion on p10). While the association is clear, the causal linkages are not, particularly for milder levels of CI, where the causal relationship between symptoms, performance and ocular signs are extremely complex and poorly evidenced. Further research is ongoing in the US to answer this question, but results are so far unpublished. At the moment, all we can do is report associations, but avoid implying causality until it is proven.
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
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No

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