Reviewer's report

Title: Vision problems linked to reading difficulties in a cohort of European school children: prismatic correction offers the most effective treatment

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Reviewer: Gladys Mitchell

Reviewer's report:

This manuscript reports the results of a comparison of two treatments for convergence insufficiency in a cohort of children with reading difficulties and average or above average intelligence. In my opinion, the title of the manuscript is incongruent with the findings reported within. This manuscript does not set out to link vision problems to reading difficulties. In addition, the title indicates that base-in prism correction is the most effective treatment but, for each outcome except total reading time, there was a significant improvement in both the base-in prism and HTS treatment arms.

The 1st sentence of the abstract states that this manuscript will “evaluate the visual status of... and compare two treatment modalities for convergence insufficiency.” In my opinion, there is no data presented that would allow the reader to evaluate the visual status of this group of subjects. This would require comparing the means at the first visit with some established normative values. The good news is that the final paragraph in the background section correctly states the goal of this manuscript and that text should also be used in the abstract.

The statistical analysis section is poorly written. I would prefer to have the information on statistical methods used in the manuscript reported here, not in the results section. I would also like to see information on how the sample size of 51 per group was determined.

The results section is also sparse and contains very little data. In my opinion, the reader should not be expected to jump between the text and tables to assess the results of the study. I would like to see some of the data (means and standard deviations) reported in the text.

With respect to the statistical methods, the choice of performing an ANOVA for each treatment arm is a bit piecemeal. In total the authors have performed 24 ANOVAS (3 groups compared with respect to 8 outcome measures). The greatest power to detect differences occurs when the degrees of freedom associated with the estimated mean square error (MSE) are maximized. This would be achieved by using a 3 group x 2 time point repeated measures ANOVA for each of the 8 outcome measures. The good news is that such an analysis method would allow for comparisons between groups. Of course, this would also mean that the authors would have to add text to the discussion section regarding
bias introduced by not randomizing subjects to the 3 treatment arms.

Major Compulsory Revisions:

1. It is unclear how subjects are assigned to the two treatment groups (self-selected or decision of doctor).

2. The choice of a 4 week treatment period is not well supported. This seems hardly enough time to assess the full benefit of the HTS treatment arm. According to the HTS website, most patients need 6 to 8 weeks to finish the program. Such a shorten period disadvantages the HTS treatment arm and the reasons for this short time frame given in the discussion section are not well developed.

3. The authors need to discuss the potential bias introduced by not randomly assigning subjects to the treatment arms and the use of a control group comprised of subjects who refused each treatment. What impact could the underlying rubric used to put subjects in the treatment arms have on the reported outcomes? What would be different about subjects who did not want to participate in the study and how would those differences affect the outcomes?

4. How was compliance measured? The authors need to include some description of the survey used to collect this data.

5. In the 2nd paragraph of the results section the authors report that the K-S test demonstrated that all measures are normally distributed “in both groups.” What about the control group? Comparisons are also made within this group using parametric statistical methods.

6. I don’t understand why the authors are reporting on comparisons of refractive error between the left and right eye. These results have no impact on the findings of the study. Instead, the authors should be reporting on the comparability of the 3 groups with respect to refractive error (which they actually do in the 1st sentence of the results section but with no supportive data).

7. I assume that repeated measures (one-way) analysis of variance was used to compare the first and second visit within each of the 3 treatment arms. Is that correct? As you see above, I am suggesting instead a 3 group by 2 time point repeated measures for each outcome measure.

8. The authors imply that Scheiman et al found improvements in reading speed, accuracy and intervention in their study of base-in prism (last sentence of paragraph 4 in discussion section). This is incorrect since that study did not examine reading measures.

9. I am puzzled by the author’s discussion on the selection of a 4 week treatment period. Children often wear base-in prism lenses and/or perform HTS for far longer than 4 weeks. As such, their argument that they wanted to be sure treatment did not have “a detrimental effect on visual function” seems a bit of a stretch to me.

10. Paragraph 6 in the discussion section on CI symptom surveys is unnecessary as written. It is not the goal of this manuscript to report on symptom changes in children with CI.
11. Similarly, I see no use for paragraph 8 in the discussion section. If anything, this information would be appropriate in the background as a way of introducing the outcome measures chosen for this study.

12. I think the 1st sentence in paragraph 9 of the discussion section needs to be toned down or more supporting material. Are there studies which show that CI progresses into visual anomalies which impact on visual development? Are there studies which show that CI leads to diminished educational development.

13. Since one focus of this manuscript is the observed change from visit 1 to visit 2 in each of 8 outcome measures, I would like to see box-whisker plots of the change in each treatment arm (including the control group).

Minor essential Revisions:

1. At the end of the 2nd paragraph describing the intervention there is a reference to Table 2 which should be removed.

2. It is unclear to me why the authors reference Scheiman in the 1st line of the 4th paragraph describing interventions. The same is true for the 1st sentence of the 5th paragraph in the discussion section.

3. What is “a two weekly basis”? Does that mean 2 times in a week or once every 2 weeks?

4. I would suggest the following for the compliance results: “Both parents and subjects reported that spectacles were worn > 80% of time when undertaking near vision tasks. In addition, subjects prescribed the HTS were compliant at least 80% of the time.

5. I believe the authors should report on the variability in the compliance data. How many parents and subjects reported spectacles were worn > 80% of the time? What was the distribution of responses? How many subjects were compliant at least 80% of the time? What was the distribution of responses?

Discretionary Revisions:

1. Table 2 is unnecessary and should be removed. If the authors wish to report the sample sizes for each group (other than in the text), they can add this information to Tables 3-7 in the column with the group names (i.e. column 1).

2. Is there more than one “age appropriate” version of the Salzburg Reading Test? If not, do you think that some of the improvement in the treatment arms could be related to a learning effect? I realize that no such improvement was observed in the control group but could that be an artifact of the fact that these subjects know they are not getting treatment (sort of a reverse placebo effect)?

3. I would like to see the percentages added to Table 1. With the differing number of males and females, it is difficult to assess comparability across age without percentages. I would also suggest removing the bottom line (with total sample size) on Table 1.

4. I would like to see a column with the mean and standard deviation for the change from first to second visit in Tables 3 to 7. I realize that the mean difference can be calculated from subtracting the first and second visit means
however I am most interested in the variability of the change. Plus, the reader does not have to mentally do the subtraction before comparing the changes observed in each group.

5. I don’t see the need for the current Figure 1. Since most of the subjects are emmetropic, a figure showing the distribution of refractive error in each of the 3 treatment arms is also not necessary.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.