Reviewer's report


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Reviewer: Gregory Goodrich

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General

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

This paper presents a use of SLO to training “reading” in subjects with macular disease. The literature review and introduction are appropriate. The outcomes studied are changes in logMAR acuity before and after training and at 3 month follow-up and change in “threshold character size for words” assessed at the same three time points. The topic is an important one and I was enthusiastic about reading the paper given its title and abstract. Once having read the paper my enthusiasm is somewhat tempered for the reasons stated below.

1. I found the title to be misleading. The title states it is an “Intensive short-term reading training...”. First, given that training occurred over 10 days at an hour per day I do not feel the training is “short-term”. Others (i.e., Nilsson) have shown reading training improvement using only five sessions. Second, the authors measured aspects of reading, but not reading itself. That is, they measured change in visual acuity and “threshold character span”, which are essentially letter and word recognition respectively. They also examined eye movement which is another component of reading. However, they neglected to actually assess any measure of reading (for example, reading speed, comprehension, etc.). Absent these assessments it is not possible to transfer the results to the reading task.

2. The inclusion of only five subjects makes generalization of the results difficult, especially since the primary change (in visual acuity, for example) occurred in only two subjects and relatively small changes in the remaining three. Even taking all subjects together, which showed approximately a two line improvement in ETDRS acuity from pre- to post-training, the change in acuity is within the test/retest error of ETDS charts (Kiser, et al (2005) Optom and Vis Sci 82 (11), 946-54). Thus one cannot state with absolute conviction that the improvement was due to the training. The sample size is also of concern since it precludes most statistical analysis other than the descriptive analysis provided in the article (a minimum of 10 subjects is generally needed for a simple t-test).

3. The change in threshold character size is also problematic since the improvement did not hold for all word lengths and the authors do not adequately explain why this should be the case if the training was effective.

4. The study also lacks an adequate, and very necessary, control population. The authors state that because each subject had received prior training that they served as their own control. However, the reader is not informed of what training each had received (was it the same for all) or if that training had in any way been deemed effective. In the final analysis this is not a tenable argument. Subjects in the current study received an addition 10 hours training. An equal number of patients would need to receive a control treatment (i.e., simply being asked to read using their best reading device) and then measure pre-, post-, and follow-up as was done for the experimental group.

5. A less detrimental point, but one worth correcting, is that the Post-training evaluations as described on page 12 is confusing. While it does describe the contents of table 2 the reader would benefit from a clearer organization of the data rather than a simple recitation of each subject’s behavior. Was there a clear, over-all pattern? If so, it should be stated. If not, that should be stated.

1. Is the question posed by the authors new and well defined?

The methodology employed is novel and interesting. The paper could be presented as a methodology for the study of component behaviors in reading with the cases presented as examples.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?
The work could be replicated, however the research design lacks a control population and too few subjects were studied to allow the reader to generalize the results. And, as stated above, the study examined components of the reading task, but did not examine reading per se.

3. Are the data sound and well controlled?

No, the study lacks the appropriate control group. The data are interesting, but limited by too few cases.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

In general yes, however, there are several limitations to the study that are not reported by the authors; small sample size, lack of a control group, limited measures of “reading” to mention a few.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

The primary effects of the training appear to be large in two individuals, moderate in two, and absent in one. While discussing each case in the results the discussion tends to focus on the over-all trend which is heavily influenced by the two cases. This may lead to an inaccurate conclusion. On the other hand, with more cases and a control population the conclusions may prove to be valid.

6. Do the title and abstract accurately convey what has been found?

No, see above.

Is the writing acceptable?

The writing, with the exception noted on page 12 and a few typos, is good.

Major Compulsory Revisions Summary: I would like to see this manuscript re-written as a proposed methodology to study reading related behaviors in cases of central field loss and as a possible training technique. I found the study very interesting in that regard and it stimulated a number of additional questions for me. For example, would this technique reveal differences in behavior between newly diagnosed cases of central field loss versus those with a long-standing history? Does SLO training as described result in faster reading speeds, greater fluency, less difficulty, longer durations, and/or greater comprehension in reading? I think the methodology and use of the SLO as described would be an excellent methods paper (Please see numbered comments above). As a results oriented paper it suffers from too few subjects, lack of a control population, and an incomplete assessment of reading.

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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)

The description of results noted above on page 12 should be corrected and correction of minor typos in the text.

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Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of limited interest

Quality of written English: Acceptable

Statistical review: No
Declaration of competing interests:

I declare that I have no competing interests.