Reviewer's report

Title: Use of quantification of vision by visual evoked potentials in visual disability assessment

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Reviewer: Benjamin Thompson

Reviewer's report:

This manuscript reports a correlation between visual acuity and the amplitude of the pattern reversal VEP in a combined group of observers with normal vision and observers with amblyopia. The authors argue that this correlation can be used to estimate acuity in patients with a range of different types of visual deficits and detect patients who are malingering in order to obtain an insurance payment on the basis of reduced visual function.

The idea of trying to determine the relationship between VEP amplitude and acuity by combining data from patients with and without reduced acuity is interesting. In addition, the second stage of investigating whether this relationship holds for a separate group of patients with a different cause of visual acuity loss (in this case optic neuritis) is well thought out. However, I am not convinced that the data presented in this manuscript are sufficiently robust to support all of the author’s claims and to provide a basis for identifying malingerers. I have detailed these concerns in the comments below:

Major compulsory revisions:

1) The manuscript is very difficult to follow in its current form and requires significant editing for grammar and structure throughout, including a revision of all subheadings and a much clearer explanation of the methods and results. Errors are also present in the title and abstract.

2) The assumption that VEP amplitude will be affected in the same way by all causes of visual acuity loss needs to be clearly justified within the manuscript.

3) The sample size is rather small for a study with the aim of providing a protocol for detecting malingerers. In addition, it is not clear how the authors selected the cases that were used to derive their correlations. It is stated that the cases were collected over a 2.5 year period but the inclusion criteria are not clearly presented. Also, what were the criteria for presuming that a patient was malingering with regard to amblyopia (“clinical evaluations section”)? These details are important as excluding patients will affect the correlations reported.

4) It is not clear to me why both pattern and flash VEPs were collected and where these different types of VEPs fit into the analyses presented later in the manuscript. This should be clarified.

5) There is no need to present results in the text and in a table. It would also be informative to present the VEP data for the amblyopic and normal groups
separately and provide clinical characteristics for the group with amblyopia including types of amblyopia and the range of visual acuity.

6) There appear to be large differences in the age ranges of some of the different groups of patients. It would be useful for the authors to comment on whether this might influence their results.

7) The results section is very difficult to follow, however it would appear that the linear fit to the combined control and amblyopia data allows for a prediction of VA in the optic neuritis group based on their VEP amplitudes. The authors point out that the predicted VAs do not significantly differ from the measured VAs for this group. Although this is true, there is still only a 7% probability that the difference between these two measures is due to chance. This is important in the context of using the predicted values to determine malingering and suggests that while the correlations are statistically reliable, they may not be strong enough to predict VA in individual cases. A suggestion of a different way to interpret the data is provided in the next comment.

8) An inspection of figure 1 seems to show two clusters of data, one with VEP amplitudes in the 8-22uV range that contains all of the observers with a LogMAR of 0 and one with much lower amplitudes. If the authors plot the distribution of the VEP amplitudes, is the distribution bimodal? If so then from the data shown in figure 1, it seems likely that one of the lobes of the distribution would include only patients with reduced VA. Therefore rather than trying to use a linear fit to determine VA the authors could consider using a cutoff VEP amplitude to detect malingeringers. Either way the authors should comment on the wide spread VEP amplitudes associated with normal visual acuity in Figure 1.

9) It is not clear to me how the case studies relate to the VA measurements. They all seem to have very poor VA so it is not surprising that their VEP amplitude is low. This should be clarified. I also found the discussion very difficult to follow. Highlighting the key conclusions from the study at the start of the discussion would be useful.

10) Some of the references used are rather obscure. There is a large literature on VEPs and visual function that could be cited more widely to support the arguments made by the authors.

Minor essential revisions
1) I’m not sure what “secondary gain” means in relation amblyopia. This should be clarified.

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

**Quality of written English:** Not suitable for publication unless extensively edited

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**
'I declare that I have no competing interests'