Reviewer’s report

Title: The effect of coaching on the simulated malingering of memory impairment

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Reviewer: Martin Rohling

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BioMed Central Neurology MS# 1384429768189208

“The Effect of Coaching on the Simulated Malingering of Memory Impairment” by Russeler, Brett, Klaue, Sailer, & Munte.

Thank you for allowing me to review this paper for publication in the BMC Neurology open access journal. I apologize for taking longer than expected to complete my review. I hope this did not cause the authors or editors too much trouble.

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

The authors do a good job at detailing the history of research in the area of symptom validity, symptom exaggeration, and poor effort. The Introduction is easy to read and logically sets up the research hypotheses and study design.

2. Are the methods appropriate and well described?

The methods are well described and sufficiently details that one could readily replicate the design if desired.

3. Are the data sound?

The data collected are sound; however, I suggest a few minor essential revisions.

1. Most of the results appear highly skewed, which is common in this type of research. Therefore, parametric inferential statistics (F test) are not usually appropriate. Nonparametric analyses are needed when the distributions are not normally distributed, with equal variance, and independently sampled. Granted, parametric inferential statistics are often robust to violations of the stipulations, but I would recommend the authors run the equivalent nonparametric analyses and report any differences in outcome from those detailed in their results section and tables.

2. The probability levels (p) for multiple comparisons need not be adjusted as stringently as the authors appear to have done (Scheffe’-contrasts). Not all pair-wise comparisons are of interest and a less conservative method, such as that proposed by Tukey could be substituted. Because the authors hypothesized certain findings, they need not adjust the probability levels to the degree that is
common when one is “data snooping” or when the direction of the effect is unknown (e.g., 1-tailed vs. 2-tailed t-test).

I have one major compulsory revision.

3. It is not clear to me how Table 3 was generated. There needs to be a better explanation for the meaning of sensitivity and specificity. Generally, the control group is used to generate estimates of specificity and the experimental or clinical sample is used to generate estimates of sensitivity. How the control group can be listed in the sensitivity column under these circumstances? Furthermore, the authors should add some calculations related to positive predictive value (PPV), negative predictive value (NPV), overall percent correct classification rate (OPC), and base rate (BR) to better put their findings into the context of clinical decision making.

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

I also have a few minor essential revisions under this heading as well.

5. All data reported in the Tables should also have the same number of digits passed the decimal point.

6. Table 2 for the COMB 1 variable is missing the data for the SS group.

7. The tables list standard error for each measure. This is reasonable, but uncommon from my experience. I recommend the standard errors be replaced by standard deviations.

8. Furthermore, I think it would be of value to report a standardized mean difference effect size, such as that recommended by Cohen (d) or Hedges (g), for each variable. The control group for each comparison could be either the “best effort” (BE) group or the patient (PAT) group. I recommend the patient group.

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

The discussion of results is balanced and adequate and supported by the data. The authors appear to understand their results very well and conveyed their conclusions about the data they collected quite well.

10. Are the limitations of the work clearly stated?

The limitations of the study are well described and adequate for the research design. No changes are recommended.

11. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?

There is a clear description of prior work and how the current works fits into the overall literature as well as the authors’ program of research. No changes are
12. Do the title and abstract accurately convey what has been found?

The Title and Abstract are adequate for the purposes of conveying the nature of the study and the study results. No changes are recommended.

13. Is the writing acceptable?

The writing is clear, logical, and acceptable. However, there are a few minor essential revisions to be done. There are a few typos in the paper.

9. The authors use citations such as [25] in text rather than the names of the authors. I recommend that cited authors’ names replace citation numbers when a citation appears in text. The author names can then be followed by a citation number.

10. The abbreviations i.e. and e.g. should be followed by a comma.

11. Under the heading Cognitive Testing, there are two commas in the line detailing the subtests for alertness.

12. The Rey 15 Items test has two periods at the end of the first sentence.

I have no reason to believe that the authors engaged in any scientific misconduct, falsification, plagiarism, or inappropriate manipulation of the data for the current study.

The quality of the English writing is adequate and no changes appear needed.

I have assessed the statistical issues and do not recommend that a statistician be consulted.

I have no known conflicts of interest that might prevent me from objectively reviewing the current manuscript.

I have no confidential comments to be made to the editors.

I am unable to decide on acceptance or rejection until the authors respond to the major compulsory revisions noted. Granted, I think they will be able to easily handle these revisions, but I would like to get a chance to see how they do this before making a final recommendation for this manuscript.

Signed,

Martin L. Rohling, Ph.D.
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University of South Alabama

Level of interest: An article whose findings are important to those with closely related research interests
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.