Reviewer's report

Title: Differentiating ADHD from Oral Language Difficulties in Children: Role of Movements and Effects of Stimulant Medication.

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Reviewer: Maj-Britt Posserud

Reviewer's report:

I think the paper should be rejected for the following reasons:

1. The main author is linked to the company selling the method they are studying.

2. There are two quite different aims in the paper, and thus the paper is not doing any of these sufficient attention. Both introduction and discussion is insufficient regarding both topics. The first question is whether the Quotient can distinguish OLD from OLD/ADHD. The second is how medication changes the responses in ADHD.

3. The methodology is not adequate for any of these aims.

Why have the authors chosen to do a follow-up study over a year? Neither of the abovementioned research questions warrant that kind of follow-up time, and while follow-ups are good in many cases, here, because it doesn't help in answering any of the research questions, the reader is left wondering why on earth the authors have chosen this approach. It would have been better to choose one time point and have a larger sample, and again, as the first author is linked to the company marketing the product under study, one is left wondering as to why this methodology was chosen.

In the first part, the author states that OLD is quite different from OLD/ADHD regarding their movements, but later on when the medication effect is assessed, four of the 16 on medication come from the OLD group. To a European reviewer, this is an anachronism, as it would be impossible to have medication on a child without an ADHD diagnosis, and it sort of goes against the first statement of OLD without ADHD being something different than OLD/ADHD.

Because the paper is trying to reach over so much, the space to discuss and report the methods and the findings is insufficient. It is insufficiently described how on-off medication effects were assessed. The off procedure is described, but not the on procedure. Was it performed the same day? Another day? Before? After? Randomly crossed over? Likewise the flowchart describing the omission of study participants from 67 to 42 to 33 is strange, as the numbers add up to much more than the 67 starting out, presumably because some children are omitted for several reasons. As stated above, it would have been MUCH better to retain the 42 originally included at T1 and analyse those for OLD - OLD/ADHD differences. This way, they wouldn't have to omit the person on Strattera either. Another
strange thing is that head injury is reported as an exclusion criteria, but none of the children is excluded for this reason, and it is not stated what is meant by head injury (in my experience, most ADHD children have had some kind of head injury)

In spite of these flaws, the findings are interesting, and the method of using movement registration in addition to other clinical instruments is definetely worthwhile reporting. However, the combination of the authors being linked to the product AND the paper having serious methodological flaws, leads to my recommending rejection in its present form. To be publishable, the paper should be rewritten in two separate publications, one answering the first research question, the second answering the second. Both papers should be based on reanalysed data from T1 only, including all available that correspond to the inclusion criteria. If the authors would like to use the follow-up data, longitudinal research questions and research methods should be applied and addressed adequately.

4. The authors have not controlled for a very important bias in the study of the differentiation between OLD - OLD/ADHD. They may just be differentiated by severity. It is quite possible and likely that the OLD/ADHD group is more generally impaired (what were the C-GAS scores for the groups?!?) than the OLD group alone, causing more generally impaired scores on the tests. Analyses adjusting for this could have been included, such as including general intelligence as covariate. Intelligence and C-GAS scores for the groups should in the very least be reported along with other descriptive data.

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

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

Declaration of competing interests:

I declare that I have no competing interests