Reviewer's report

Title: Autonomous exercise game use improves metabolic control and quality of life in type 2 diabetes patients - a randomized controlled trial

Version: 2 Date: 9 September 2013

Reviewer: Mathias Ried-Larsen

Reviewer's report:

Thank you for the opportunity to re-review this manuscript. The authors generally did a nice job in revising the manuscript as it reads much nicer. In the revised draft, some previous misunderstanding was cleared, but raised some other (minor) concerns.

There might have been a misunderstanding in how, this referee, though the authors handled the drop-out on the primary outcome variable. As the authors state, the last observation carried forward was only applied on the secondary outcomes. However, my concerns still apply to the method;

- Cases should be missing completely at random (this needs to be demonstrated)
- Last observation carried forward ignores whether the participant's condition was improving or deteriorating at the time of dropout
- If there are more dropouts in the treatment group than in the control group, such an approach to analysis will bias results in favor of the intervention, since a greater proportion of patients in the treatment group will have their decline artificially stabilized at an earlier stage. (Thus - we do not know whether or not the patients were deteriorating before baseline - this can affect the follow-up).

Methods:

Please state more clearly, that the waiting controls are not included in the analysis after intervention (it is stated, but I was still a bit confused until a saw the flow-chart). I think this would improve this section.

Line 72-75: What was the recall period?

Results:

How are the results reported? Are the () SEM's? If they are SD's, please add CI's instead where the statistics are analytic and SD's when descriptive.

In Table 2: Please add effect size, confidence intervals and N, as it is important to see how the effect sizes change across models

Regarding the models presented in table 2 this is what I meant in the previous review (comment 13). That is analyzing the difference in change between groups. However, I wondered about the weight adjustment. One assumption in confounder control is that a confounder cannot on the causal pathway. It is possible that Wii affects weight (as a proxi for adiposity) and that would affect
Hb1Ac, right? Otherwise you’re checking for effect mediation. I suggest you leave out the particular adjustment.

Discussion:
Please discuss the potential bias introduced by applying a complete case analysis and how the large drop-out (more than 20 %) affect the interpretation of your data. I think a discussion regarding the missing is handled is appropriate.

The argument (l. 253-256) regarding no differences at baseline should be directed against a comparison between the included and the drop-outs/missing data subjects rather than a comparison of effect sizes between the ones who completed the intervention. So please check differences baseline results on Hb1Ac and other key variables between completers and drop outs - stratified by allocation and report that.

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

**Quality of written English:** Acceptable

**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