*April 1, 2015*

*Concerning IJSG-53*

*Dear dr. Ifenthaler,*

*Thank you very much for the positive and constructive review of our paper.*

*We tried to do our best and to repair all comments and useful suggestions by both reviewers (reviewer D and Reviewer A respectively).*

*Please find included in this letter our detailed answer to the points made by both reviewers.*

Reviewer D

“Regarding the literature review, using “persuasive games” as search

keyword is a bit limited since there are various games related to awareness

but not classified as persuasive, which are excluded from the survey.

Moreover, no references are given for most of the games and a sound

structuring of the survey itself would be appropriate. The goal here was

only considering games for which an evaluation on effectiveness was

available and for which there was a control group in the experiment design

(see the games summarised in the appendix). The games have not grouped

according to the features of the corresponding evaluation studies: for

example, the authors did not mention which sample of people was selected in

the experiments, if there was a specific target user, if such evaluation was related somehow to game design features, if games were assessed per se or in a wider setting. Consequently, authors are asked to revise section 2.1 and give a deeper synthesis in their state of the art”.

*We added all keywords that were used for searching relevant literature. We are rather sure that we did not miss many empirical studies.*

*We checked once more the references of the games. They are mentioned, and also included the table of the Appendix.*

*We changed the description of the review in section 2 by emphasizing the focus of the review (effects and methodology) (page 3). We emphasized that in the context of this study we were mainly interested in the (statistical and internal) validity (Cook & Campbell, 1979, Quasi-Experimentation: Design & Analysis Issues for Field Settings) of the studies found (answering the question is there really an effect that can be attributed to the game, and not something else?). Otherwise a completely different paper should have been made (see e.g. Wouters & Van Oostendorp, Computers & Education, 2013; Wouters, Van Nimwegen, Van Oostendorp & Van der Spek, J. Educ. Psy, 2013 for the methodology of meta-reviews of game research).*

*However we structured the outcomes of the review by including a table of the effects found in the different studies to make it more easy to see what the different effects were (page3).*

“The evaluation study proposed in the second part of the paper goes beyond

the previous study on the same game (ref. [30]) both for the content (i.e.

awareness/knowledge of micro-macro scale) and for the introduction of a

control group subject to a similar learning content but in a non-game

setting (a powerpoint presentation, in this case). The progress in awareness and knowledge have been measured by pre/post test questionnaires (including likert scales and open questions). The authors verified no big difference in exposing users to similar content with different media.

Such a study is limited because of the small and homogeneous sample of

people involved and the very similar learning content for the two groups.

The authors are well aware of the various limitations of their approach;

nevertheless, they are asked to comment explicitly in the text on the

following aspects”.

*We agree that the sample is not large, but the controlled situation of the current study comes at a cost. As said in the paper the sample size and reliability of measuring instruments is high enough to be able to detect significant differences. We preferred in our study to have high reliability and validity (e.g. we controlled what participants are doing during the study, assured equal conditions for all participants etc.) instead of a large sample size at the cost of reliability and validity. And just because the sample size is not that large, it is better to have a rather homogeneous sample (otherwise we don’t know anything at the end of the study). In the Discussion section (6.0 and 6.1) we discussed this point more explicitly.*

“he difference between the number of students involved in the primary study

(ref. [30]), and in the present one, i.e. 653 vs. 46, is not as irrelevant

as the authors state, even if statistical minimal requirements were

respected”.

*See above; we expended our argument on this point in the text (page 13 and 14).*

“The homogeneous target of people considered may affect the results of the

assessment. For example, the fact that students were probably already aware

of ecological themes makes the progress in awareness/knowledge less evident”.

*It would be interesting to do a replication study with a less educated group of participants or older aged participants. The text is changed in this respect (p 14) and added to the implications for further research (6.1).*

“Moreover, the social desirability bias in which respondents answer what is

socially acceptable rather than what they actually think is an interesting

aspect, but this should affect more the awareness than the knowledge

(behavioural change was not included in the test, anyway)”.

*We agree with this observation, but it should be noted that the control condition even acquired more knowledge, so no advantage for the game condition.*

“The time for playing/reading was only 20 minutes: the attention of the user can stay high in this short time and the state of flow, which is usually better achieved in games, might not influence that much in this case. Graphics and interaction are generally considered a vantage point of games and able to catch the attention of the player, but the authors consider those only distracting”.

*We included this point at page 12. We had not the intention to expect with this game that it would be only distracting, on the contrary. We took this game because positive results were reported in the literature.*

*At page 14 we mention the time aspect.*

“Generally speaking, the crucial point is not that the game is intrinsically

more motivating than text (it also depends on how the user is used to/like

videogames) but a well-designed game can be more motivating than a simple

text. No comment has been given on the game quality, even also from the

player’s point of view: some questions of the test were related to the

appraisal of the game, but the authors did not discuss this aspect in the

paper.”

*We included a sentence (page 5) that just because preceding studies with Enercities showed positive effects, and the prices received, that it is probably a well-designed game with stimulating game features. That was for us to the reason to take this game as research topic.*

O*n page 11 at the section Evaluation measures (5.4) we report the data concerning perception of the games vs the powerpoint, and in the Discussion page 12 we discuss the fun aspect of the game and that of the powerpoint, which was more or less similar which could have been explained by the lack of difference in change of attitude.*

“On purpose, very similar content has been created for the game and for the

presentation.”

*Yes, that was for our research questions and study essential.*

“On the one hand, this allows a better comparison of the

results; on the other hand, in addition to the short time for

playing/reading, this choice can flatten the differences in the results. No

discussion on game features and game mechanics have been proposed in this

work (in fact, this was out of the scope) except for supposing some

distraction due to the graphics/interaction. Oppositely, those factors can

be the ones which can boost the motivation and then produce more awareness

(rather than more persuasion), at least for frequent players, as the young

usually are.

Finally, the presented outcomes do not surprise at the light of the design

choices of the experiment. Possibly, such an overall evaluation is not the

crucial research question here, unless it is considered in stricter contexts and with limited objectives. As the authors themselves mention, evaluating the game within a richer experience (as in [30]), would be more appropriate for persuasive games: using games together with other educational tools/settings would possibly increase the effectiveness in awareness and knowledge thanks to integration of the different media and the longer exposure to the learning content”.

*Yes, we agree with this point of research context. We already mentioned that on page 13: contextual factors can be boosters of game effects. However, at the same time it implies that the game effect itself is apparently not that big and that was the point of our study. We added this to the paper.*

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Reviewer A:

“More detailed comments are as follows:

1. The first passage of the Introduction shall be deleted – this is very confusing.”

*We clarified the function of the first paragraph (page 1).*

2. “The title needs to be more detailed”.

*We specified the title of the paper*

3. “A more precise definition of persuasive games is necessary”.

*We added more precise information, e.g. definition, of persuasive games (page 1-2).*

4. “The reader would expect a more precise research question and hypothesis as well a clear presentation of the research methodology”.

*We paid attention to sharpening the research question, hypotheses and methodology (design). Page 6 includes a new paragraph for this. And on page 2 we made the research questions more explicit.*

5. “The Conclusions are too abbreviated. Many of the obvious limitations of the presented work need to be addressed. Implications for theory, methodology and practice need to be more elaborated”.

*We restructured the conclusions (1 to 3).*

*Furthermore, we elaborated the Limitations and Implications section, and listed on page 14 (7) recommendations.*

*We hope that we with all changes and additions do meet the requests. In our view the paper has been improved considerably, thanks to the useful comments of the reviewers.*

*Looking forward to your decision,*

*With best regards*

*Herre van Oostendorp*

*Mara Soekarjo*

*Utrecht University*