## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (http://bmjopen.bmj.com/site/about/resources/checklist.pdf) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

## **ARTICLE DETAILS**

TITLE (PROVISIONAL)	The Brain Games study: Protocol for a randomised controlled trial of
	computerised cognitive training for preventing mental illness in
	adolescents with high-risk personality styles
AUTHORS	Mewton, Louise; Hodge, Antoinette; Gates, Nicola; Visontay,
	Rachel; Teesson, Maree

## **VERSION 1 – REVIEW**

REVIEWER	Dr. Claire M Kelly
	Mental Health First Aid Australia
	Deakin University Australia
REVIEW RETURNED	30-May-2017

GENERAL COMMENTS	Overall I think this is a great project. Interventions which are cheap,
	carry few risks and are easy and cheap to distribute are very
	important, and I wish the researchers well.
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	I do have a couple of concerns.
	I've not been able to find any published data on using the SDQ for
	young people aged 18+. Although instructions are provided for
	scoring it for those over 18, given that there does not appear to be
	any validation for the scale for young adults (unless, I would like to
	know why this measure was chosen. It's a rich, multi-dimensional
	tool with multiple scores in different areas so I can see the appeal.
	However, the main argument at this point for using it seems to be
	that a similar pilot study (which appears to have been on 15
	adolescents) did the same, and I think a better argument needs to
	be put forward. While I acknowledge that the main outcome will be
	the difference between pre-, post- and follow-up assessments, these
	changes may not be very meaningful without evidence the scale is
	valid for young adults. Is there scope for using the MINI and the
	MINI-KID as in the eligibility interviews? I'm less familiar with these
	but the indication that the authors intend to use the lifetime version
	does suggest that there are other options.
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	I'm concerned about the sample size. An hour a day of tasks, five
	days a week for five weeks seems to be a lot, and I'm concerned
	that 30% attrition is not a realistic estimate. This is a really intensive
	intervention.
	Some questions worth exploring:
	Is Lumosity able to provide data on engagement? How many
	people currently use the site for an hour a day 5 times a week?
	What is the natural attrition on their existing programs?
	2. Is there budget to allow for even 50% attrition?

- 3. Is there scope to find out how much time the eligible participants currently spend playing games? If this is a major change for them (rather than a switch from one game to another) this is a risk for the project. If they are currently playing a different game and this would be a relatively simple change that is one thing, but fitting in a full hour of a new activity every day for five weeks could be a big challenge for some.
- 4. Can the authors define attrition/completion? Will a participant have to complete all 25 hours in order to be considered a completer? Given the ITT analysis, any participation could contribute to the final analysis, I know, but I'm wondering about the definition as it was operationalised when calculating the power estimates.

The plain language statement needs to state that they will be contacted if they miss sessions. A text after one missed session and a phone call after 2 may be experienced as intrusive by some, but particularly if there is no warning about it. The plain language statement also needs some clarity under the heading "Do I have to take part". It needs to clearly state that participation is voluntary before going into the additional detail which is already there.

I wish the authors the very best with this project. Self-guided interventions are going to continue to be very important going forward. Given the natural engagement with these sorts of activities, and the current lack of evidence, research of this sort is certainly needed.

REVIEWER	Evanthia Sakellari
	Technological Educational Institute of Athens, Greece
REVIEW RETURNED	11-Jun-2017

#### **GENERAL COMMENTS**

The paper is concerned with a research protocol which is about an interesting study in the field of prevention of mental illness. There are some issues that authors need to address before possible publication:

- The authors need to reconsider their goals, especially in regards to examine their intervention in reducing psychopathology (maybe this is to overambitious). It is understandable what they are looking for, however, it is suggested that they revise it.
- In the study design, the authors state only one measurement (SDQ), however, later in the paper, more tools are described. In this way, it is not clear for the reader what they would like to measure. It is suggested that they check it and revise it accordingly.
- The author state that the SDQ is measurement for psychopathology, however, the authors who have developed it state that it is a brief behavioral screening questionnaire.
- In the participants and recruitment part, the authors describe the inclusion criteria, but it is not clear for the reader how they will assess the first inclusion criterion.
- The online informed consent needs more explanation. It is also suggested that the authors describe all the ethical issues in one section of their paper.
- The WHODAS 2.0 will be used to collect data, however, this is a tool for adults and is not developed to be used online. It is advised that the authors support their decision for the reader.

REVIEWER	Lisbeth Homlong
	The Norwegian Board of Health Supervision/
	Kurbadet Family Practice
	Norway
REVIEW RETURNED	22-Jun-2017

GENERAL COMMENTS	I find the protocol manuscript very well written, the English Language is fluent. The planned study is thoroughly descibed in the protocol and the study plan seems to be well structured and organized. The Project is interesting and uses modern Technology to gain knowledge on an important Field. The theories behind the idea of using computerized cognitive training in enhancing executive functions to prevent mental illness, are based on recent research on brain Development in Young people. As stated in the introduction, Mental Health problems in Young People are common world wide and constitute a major burden of disease in adolescence and Young adulthood. The Project is thus highly relevant. I have no major concerns about the study protocol. I have some minor concerns, though, on attrition. You plan to recruit 200 persons. You need a sample size of n=140. I would expect about 50% attrition in a Project like this, I would therefore suggest that you try to recruit at least 280 persons in order to account for potential attrition. I also have some concerns on the recruitment procedure and if the recruited persons will be representative. I miss some reflections on that in the study protocol, in the section describing potential weaknesses of the study. You may have gotten a more representative Group of study subjects if you chose to recruit persons in a high School or college population.  I also miss some reflections on the practical use of the online training program if the results show a significant effect on preventing mental illness in high risk individuals. Where and how can such a program be implemented? In Schools? In the School health services? In youth Health clinics? Other?

### **VERSION 1 – AUTHOR RESPONSE**

Reviewer: 1

Reviewer Name: Dr. Claire M Kelly

Institution and Country: Mental Health First Aid Australia, Deakin University, Australia Competing

Interests: None declared

Overall I think this is a great project. Interventions which are cheap, carry few risks and are easy and cheap to distribute are very important, and I wish the researchers well.

I do have a couple of concerns.

I've not been able to find any published data on using the SDQ for young people aged 18+. Although instructions are provided for scoring it for those over 18, given that there does not appear to be any validation for the scale for young adults (unless, I would like to know why this measure was chosen. It's a rich, multi-dimensional tool with multiple scores in different areas so I can see the appeal. However, the main argument at this point for using it seems to be that a similar pilot study (which appears to have been on 15 adolescents) did the same, and I think a better argument needs to be put forward. While I acknowledge that the main outcome will be the difference between pre-, post- and

follow-up assessments, these changes may not be very meaningful without evidence the scale is valid for young adults. Is there scope for using the MINI and the MINI-KID as in the eligibility interviews? I'm less familiar with these but the indication that the authors intend to use the lifetime version does suggest that there are other options.

>>>>>>>>>>>>Validation studies of the young adult SDQ have been published using special populations only (please see refs 34 and 35 included in the revised manuscript). We have been in contact with a research team who have investigated the properties of the young adult SDQ in a large Australian sample (n=1180). This study, which is currently under review, showed that the young adult SDQ functions similarly to the adolescent version. We have also been in contact with the developers of this tool who have confirmed that while it has been used extensively in practice, no validation studies have been published yet, although they know of studies that are close to publication. We are therefore confident that this instrument is meaningful for young adults, despite the fact that no validation studies have been published at present. In the revised manuscript, we now provide more information around this, as well as more information about the strengths of this tool and its reason for inclusion in the study.

(pg. 13-14 of revised manuscript): "The SDQ is also brief and multidimensional, providing information across a wide range of psychopathology as required for the current study. An online version of this questionnaire will be developed for the current study in consultation with the original SDQ developers. Recently, a young adult version of the SDQ has also been developed, with minor changes to the wording and scoring of the adolescent instrument. According to the developers of this instrument, it has been used extensively in practice but validation studies of this instrument have only been published in special populations at present [34, 35]. The young adult version will also be used to assess psychopathology in those aged 18 years and over in the current study to allow continuity of measurement across the full sample."

I'm concerned about the sample size. An hour a day of tasks, five days a week for five weeks seems to be a lot, and I'm concerned that 30% attrition is not a realistic estimate. This is a really intensive intervention.

Some questions worth exploring:

- 1. Is Lumosity able to provide data on engagement? How many people currently use the site for an hour a day 5 times a week? What is the natural attrition on their existing programs?
- 2. Is there budget to allow for even 50% attrition?
- 3. Is there scope to find out how much time the eligible participants currently spend playing games? If this is a major change for them (rather than a switch from one game to another) this is a risk for the project. If they are currently playing a different game and this would be a relatively simple change that is one thing, but fitting in a full hour of a new activity every day for five weeks could be a big challenge for some.
- 4. Can the authors define attrition/completion? Will a participant have to complete all 25 hours in order to be considered a completer? Given the ITT analysis, any participation could contribute to the final analysis, I know, but I'm wondering about the definition as it was operationalised when calculating the power estimates.

>>>>>>>>Recruitment for this study has commenced and the estimated attrition rate based on the current data is approximately 37%. In the manuscript, we have revised the number we aim to recruit to n=220 to account for attrition based on this data. Please also note that because the rates of attrition are expected to be relatively high, we now also include a secondary per protocol analysis in addition to the intention to treat analyses.

(pg 16 of the revised manuscript): "Given the high rates of attrition expected, secondary analyses will also be conducted for all outcome variables on a per-protocol basis."

>>>>>>> The control and intervention programs consist of 10 games and these games need to be completed within each session. The hour per session was based on estimates from Lumosity and appears to be an over-estimate based on current data from the study (sessions tend to take ~30-40 minutes). The manuscript has been edited to more accurately reflect the structure of the program and the time commitment involved.

(pg. 11 of revised manuscript) "Training for both conditions will consist of an intensive program of ten games per day (~30-40 minutes), five days per week, over five weeks."

The plain language statement needs to state that they will be contacted if they miss sessions. A text after one missed session and a phone call after 2 may be experienced as intrusive by some, but particularly if there is no warning about it. The plain language statement also needs some clarity under the heading "Do I have to take part". It needs to clearly state that participation is voluntary before going into the additional detail which is already there.

I wish the authors the very best with this project. Self-guided interventions are going to continue to be very important going forward. Given the natural engagement with these sorts of activities, and the current lack of evidence, research of this sort is certainly needed.

Reviewer: 2

Reviewer Name: Evanthia Sakellari

Institution and Country: Technological Educational Institute of Athens, Greece Competing Interests:

None declared

The paper is concerned with a research protocol which is about an interesting study in the field of prevention of mental illness. There are some issues that authors need to address before possible publication:

- The authors need to reconsider their goals, especially in regards to examine their intervention in reducing psychopathology (maybe this is to overambitious). It is understandable what they are looking for, however, it is suggested that they revise it.

>>>>>>>The study aims have been justified in the introduction and were formulated on the basis of pilot studies that indicate that cognitive training may be useful in preventing psychopathology in at risk young people.

- In the study design, the authors state only one measurement (SDQ), however, later in the paper, more tools are described. In this way, it is not clear for the reader what they would like to measure. It is suggested that they check it and revise it accordingly.

>>>>>>>>>> In the original manuscript, under the study design section, the SDQ is listed as the primary outcome. Secondary outcomes are then listed in the following sentence and the measurement of these described in more detail under the "Measures" section. We have revised this sentence to make it clear that the measurement of the secondary outcomes is described in more detail below.

(pg 9 of the revised manuscript) "Secondary outcomes will include executive functioning ability as measured by online neuropsychiatric tasks, as well as day-to-day functioning and alcohol consumption as measured by standardised measures listed below."

- The author state that the SDQ is measurement for psychopathology, however, the authors who have developed it state that it is a brief behavioral screening questionnaire.
- >>>>>>>>>>>>According to the original validation study (see Reference 25 in the original and revised manuscript) the SDQ is described as a brief measure of "prosocial behaviour and psychopathology" by the authors who developed this tool.
- In the participants and recruitment part, the authors describe the inclusion criteria, but it is not clear for the reader how they will assess the first inclusion criterion.

>>>>>>>>> We have now made it clear that assessment of inclusion criterion 1 will be done via the Substance Use Risk Profile Scale (SURPS) which has been described in detail under the measures section.

(pg 9 of the revised manuscript): "1) at high risk for development of a mental illness based on elevated levels of personality risk factors, including hopelessness, anxiety sensitivity, impulsivity, and sensation seeking [as measured by the Substance Use Risk Profile Scale (SURPS), described below]"

- The online informed consent needs more explanation. It is also suggested that the authors describe all the ethical issues in one section of their paper.
- >>>>>>>>>Please find the online consent form appended to this submission. We apologise for not including this in the first submission. Both consent forms, and the procedures, have been approved by the University of NSW Human Research Ethics Committee. All information about consent is now included in one section as requested (please see revisions to recruitment section, pg 10-11 of revised manuscript)
- The WHODAS 2.0 will be used to collect data, however, this is a tool for adults and is not developed to be used online. It is advised that the authors support their decision for the reader.

and over in the Australian population [36, 37]."

Reviewer: 3

Reviewer Name: Lisbeth Homlong

Institution and Country: The Norwegian Board of Health Supervision/ Kurbadet Family Practice,

Norway Competing Interests: None declared

I find the protocol manuscript very well written, the English Language is fluent. The planned study is thoroughly descibed in the protocol and the study plan seems to be well structured and organized. The Project is interesting and uses modern Technology to gain knowledge on an important Field. The theories behind the idea of using computerized cognitive training in enhancing executive functions to prevent mental illness, are based on recent research on brain Development in Young people. As stated in the introduction, Mental Health problems in Young People are common world wide and constitute a major burden of disease in adolescence and Young adulthood. The Project is thus highly relevant. I have no major concerns about the study protocol. I have some minor concerns, though, on attrition.

You plan to recruit 200 persons. You need a sample size of n=140. I would expect about 50% attrition in a Project like this, I would therefore suggest that you try to recruit at least 280 persons in order to account for potential attrition.

>>>>>>>Please see response to Reviewer 1 above who has cited a similar concern. Based on current attrition, we expect an attrition rate of about 37% and the numbers needed to recruit have been amended to n=220 in the revised manuscript.

I also have some concerns on the recruitment procedure and if the recruited persons will be representative. I miss some reflections on that in the study protocol, in the section describing potential weaknesses of the study. You may have gotten a more representative Group of study subjects if you chose to recruit persons in a high School or college population.

>>>>>>>>>>> We now cite a recent review that supports the use of social media for recruitment (please see reference 27 in the revised manuscript) and justify our recruitment strategy in the revised manuscript.

(pg 10 of revised manuscript): "Recruitment through social media has been shown to be effective and cost efficient, with obtained samples of similar representativeness as those recruited via traditional methods [27]"

I also miss some reflections on the practical use of the online training program if the results show a significant effect on preventing mental illness in high risk individuals. Where and how can such a program be implemented? In Schools? In the School health services? In youth Health clinics? Other?

>>>>>>>The final paragraph in the discussion of the original manuscript included reflections on the practical use of this program in schools.

This paragraph has been amended so that it is clear that this is where such research may be eventually practically applied. (please see amendments to final discussion paragraph)

# **VERSION 2 – REVIEW**

REVIEWER	Dr. Evanthia Sakellari
	Assistant Professor, Department of Public Health and Community
	Health, Athens University of Applied Sciences, Greece
REVIEW RETURNED	22-Jul-2017
GENERAL COMMENTS	The authors have addressed all my comments. This is a very
	important study with implications in practice. I wish to them all the
	best.
REVIEWER	Lisbeth Homlong
	The Norwegian Board of Health Supervision/Kurbadet Family
	Practice, Norway
REVIEW RETURNED	01-Aug-2017
GENERAL COMMENTS	I am satisfied With your replies to my previous comments, in my first
	review. I have no further comments or questions. I recommend
	publication.