Author's response to reviews

Title: Secondary Outcomes of a School-based Universal Resiliency Training for Adolescents: A Cluster Randomized Controlled Trial

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Version: 3

Date: 9 September 2014

Author's response to reviews: see over
Dear Miss Tak,

Your manuscript has now been peer reviewed and the comments are accessible in PDF format from the links below. Do let us know if you have any problems opening the files.

Referee 1:  
http://www.biomedcentral.com/imedia/1992660215138939_comment.pdf

Referee 2:  
http://www.biomedcentral.com/imedia/2047685310139389_comment.pdf

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2) As part of the process of revising your manuscript we would like to use the WebCONSORT tool which is designed to help you improve the reporting of your randomized trial. You can access the tool by clicking on the following link: http://www.webconsort.fr/registration.php?v0=E28DZac.

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Please also ensure that your revised manuscript conforms to the journal style (http://www.biomedcentral.com/info/ifora/medicine_journals). It is important that your files are correctly formatted.

We look forward to receiving your revised manuscript by 12 September 2014. If you imagine that it will take longer to prepare please give us some estimate of when we can expect it.

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With best wishes,

Victorino Silvestre  
Journal Editorial Office  
BioMed Central

E: editorial@biomedcentral.com  
W: www.biomedcentral.com  
on behalf of Dr Sara Evans-Lacko
Dear Editor,

First of all, we would like to thank the editor and the reviewers for the time and effort they have spent on reading and commenting on our work. We are grateful to be granted the opportunity to revise the manuscript and we believe that by following the reviewers suggestions we have improved the manuscript. We hope we have addressed all issues to the satisfaction of the reviewers and the editor.

Referee 1:

Reviewer’s report
Title: Longitudinal Outcomes of a School-based Universal Resiliency Training for Adolescents: A Cluster Randomized Controlled Trial
Version: 2
Date: 7 August 2014
Reviewer: Patrick Pössel

Reviewer’s report:
The manuscript “Longitudinal Outcomes of a School-based Universal Resiliency Training for Adolescents: A Cluster Randomized Controlled Trial” describes a 2-year follow-up randomized control group prevention study with 1341 8th grade students. In this study, a Dutch version of the Penn Resiliency Program is compared to a no-intervention control condition. The program promotes cognitive coping over the 2-year follow-up but has no impact on anxiety, depression, happiness, hopelessness, or life satisfaction. Based on the response letter of the authors, I conclude that this manuscript already underwent a first round of reviews. The topic of the manuscript is relevant, and the manuscript has many positive aspects including an appropriate sample size, sophisticated analyses, and a long-term follow-up. Thus, I have only a few minor recommendations.

Before I provide some detailed comments, I would like to congratulate the authors on publishing non-significant findings. Such findings are particularly difficult to publish but crucial for the development of our understanding of prevention and for the future of behavioral health care.

Major Compulsory Revisions
1) While not all n.s. findings can be explained by the reliability of the scale scores, I suggest to consider adding possible issues with the reliabilities of some of the scale scores in the Discussion section. Happiness was measured with one item and the reliability of the LOT-R scores is low. Further, while I am not an expert on how to measure academic achievement, I wonder about the reliability of student-reports of their last test. I worry about the student report but also about the possible error associated with measuring the grade in one test. Thus, I suggest to add information about the reliability of student-reported test results and the correlations of such measures with more general measures of current academic achievement (e.g., grade in the same subject for a whole semester or school year). Depending on the reliabilities and correlations the authors find, they should consider adding this issue in their discussion of the n.s. findings. Finally, I am concerned about the self-report character of alcohol consumption, smoking, and truancy. I am particularly concerned about the reliability of the first two variables as my understanding is that it is illegal for 14-year old adolescents in the Netherlands to drink or smoke. Thus, references supporting the reliability of self-report data would be helpful and/or a discussion of these issues is suggested.

Reply:
The reviewer rightly points to issues regarding the reliability of some of the measures that we have used. We therefore added a more elaborate discussion of the implications of the limitations of our measures to the discussion section. With regard to happiness and optimism, we added the following:

For happiness on p. 20 Discussion, line nr. 479-482:
‘Second, the measure used for happiness consisted of a single item, which might be less reliable and valid in comparison with a multiple item questionnaire, although this measure was used reliably in previous research’

For optimism on p. 20 Discussion, line nr. 487-488:
‘In light of this low reliability, the results should be confirmed before firm conclusions can be drawn.’

Concerning school grades, a single test score will not always correspond to the semester grade for that subject. Therefore, using this test score as the average grade for the subject is not the best way. Unfortunately, we did not receive the permission to use the official school grades. In order to give an impression about the effectiveness of OVK on school grades, we used the self-report method. To address the low validity of the current manner to assess school grades, we emphasized in the discussion that the results concerning school grades should be interpreted with caution.

On p. 20 Discussion, line nr. 490-492 we added the following:
‘Besides, the latest grade does not always reflect the average grade for a specific subject. Therefore, the results concerning school grades should be interpreted with caution.’

Although it is illegal for adolescents younger than 18 to buy alcohol or to carry alcohol outside the home environment, it is not illegal to drink alcohol in private places or at home. Previous research has shown that alcohol use and smoking can be accessed through self-reports in a reliable and valid way in an adolescent population (Brener et al., 2002; Dolcini, Adler, Lee, & Bauman, 2003). We also expect that the same holds for truancy. In addition, adolescents are ensured that their answers are treated confidentially.

On p. 20 Discussion, line nr. 492-496 we added the following:
‘Using self-reports to assess alcohol, smoking, and truancy may be a limitation as well, since adolescents might feel reluctant to report using those substances or skipping a class. However, previous researchers have shown that self-report is a reliable and valid way to assess smoking (Dolcini, et al., 2003) and alcohol use (Brener, et al., 2002). Moreover, adolescents were assured their answers would be handled confidentially.’

Minor Essential Revisions
2) In the Abstract, the authors state “The intervention and control condition consisted of four schools and 655 adolescents, and five schools and 735 adolescents, respectively. The total sample consisted of 1341 adolescents...” I am a bit confused as 655 + 735 is not 1341. Thus, I would like to ask the authors to clarify the reason for the difference between 1390 and 1341 in the Abstract.

Reply:
We agree with the reviewer that these differences in numbers might be confusing and we therefore omitted the information that we included 1390 participants initially. Instead, we only reported the number of adolescents who actually participated in the study. In the results section and the flow-diagram the flow of participants through the study is explained in more detail.

p.2 Abstract, line nr. 38 - 39:
‘The intervention and control condition consisted of four schools and 655 adolescents, and five schools and 735 adolescents, respectively.’

Changed into:
‘The intervention and control condition consisted of 634 adolescents from 4 schools and 707 adolescents from 5 schools, respectively.’
3) In the Results section, the authors state that 12 schools were interested in participation but only nine did in fact participate. Please explain what happened to the three schools that were not included. If more schools agreed to participate then needed, please describe how the schools that participated were selected.

Reply:
The three schools that did not participate were interested at first, but did not want to continue further participation in the research project due to various reasons (too busy at school with other projects, or they had the impression that 2,5 years of research would take too much effort from the staff).

p. 13 Results, line nr. 314 -315:
‘From the 79 schools that were approached to participate in this project, 12 schools showed interest and nine schools participated.’

Changed into:
‘From the 79 schools that were approached to participate in this project, 12 schools showed initial interest, and nine schools eventually agreed to participate.’

4) Finally, two of the main strengths of the manuscript – the long follow-up and the LGCM – might be a reason why the authors were not able to find significant results. A latent growth model does not automatically provide information about changes over time periods shorter than the final time point (e.g., 6, 12, or 18 months). Thus, it is possible that OVK was effective over shorter time intervals. While such information may seem trivial, it would not be trivial as psychotherapy research and some previous prevention studies demonstrate that the strengths of effects of psychological interventions change with the lengths of the follow-up period (e.g., Pössel et al., 2011; Wahl et al., 2014). If prevention is effective over shorter periods than 2 years, the questions we have to ask ourselves are very different from the ones if prevention has no effect at all. Considering that the authors include already many variables and analyses, I do not suggest to add analyses with the 6, 12, and 18 month follow-up as primary endpoints. Instead, I would like to ask the authors to add a discussion of the possibility of changes in the strengths of prevention effects over time.

Reply:
The suggestion that the effect of an intervention may differ over time is a very valuable one. In our own data we have examined whether there were effects of the intervention at the different follow-up assessments for the primary and secondary outcomes. No significant effects of the intervention were found at other assessment points, other than the ones that were presented in the current manuscript and the primary outcome paper. Also, the graphs for the outcome variables, including the growth curves across all the assessment points, showed that the intervention and control group did not differ significantly in their development over time. Hence, we do not have any evidence that the effectiveness of the intervention differed across assessment point. Therefore, we do not include this in our discussion.

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests: No conflict of interest.

Referee 2:
Reviewer's report
Title: Longitudinal Outcomes of a School-based Universal Resiliency Training for Adolescents: A Cluster Randomized Controlled Trial
Version: 2 Date: 12 August 2014
Reviewer: Alison Calear

Reviewer's report:
The current paper presents secondary analyses of a trial testing the effectiveness of a universal depression prevention program in schools. The following changes are recommended.

Major Compulsory Revisions:
1. Please make the aims of the study clearer in your abstract and introduction. In particular, please make it explicit that this paper focuses on secondary analyses and is not the main outcomes paper.

Reply:
We agree with the reviewer that it is important to state more clearly that this study focuses on the secondary analyses and does not present the analyses on the primary outcomes. We therefore changed the title into: ‘Secondary Outcomes of a School-based Universal Resiliency Training for Adolescents: A Cluster Randomized Controlled Trial’.

And we reframed the abstract:

p. 2 Abstract, line nr. 27 - 31:
‘Background: Investigating the long-term effectiveness of the adolescent cognitive behavioral resiliency training Op Volle Kracht (OVK) on depressive and anxiety symptoms, hopelessness, happiness, life satisfaction, optimism, coping, self-efficacy, and school functioning.’

Changed into:
‘Background: The study investigated the long-term effectiveness of the adolescent cognitive behavioral resiliency training Op Volle Kracht (OVK) on the secondary outcomes: anxiety symptoms, hopelessness, happiness, life satisfaction, optimism, coping, self-efficacy, and school functioning. In addition, the study analyzed whether the secondary outcomes moderated the intervention effect on depressive symptoms.’

The primary outcome paper is still under revision at another Journal. We expect this process to take another few months, but we hope to publish it as soon as possible.

2. Please revise your ‘Background’. At present, it is hard to follow and repetitive in parts. It is also presenting a selective overview of previous research in the field. There have been a number of comprehensive reviews of school-based depression prevention programs that would be more meaningful, and provide a more balanced overview, than selectively reporting on a few studies and giving the impression that this is the state of the field.

Reply:
The introduction section was restructured in order to address both major compulsory revisions 1 and 2, since we agree that the ‘Background’ section could be structured more clearly. In addition, we added a more comprehensive overview of the literature into school-based depression prevention programs (see the entire ‘Background’ section, p. 3 - 6). We added the following reviews:


3. Please provide a more balanced discussion of your results in light of previous research. Many school-based depression prevention programs have found significant effects and no iatrogenic effects. In addition, the most recent comprehensive review of depression prevention programs, which you referenced in your paper (Ref 20. Merry et al. 2011), found evidence for the effectiveness of universal programs. Thus, your statement that "Currently, it is questioned whether universal prevention is an effective way to prevent depression" is inaccurate.

Reply:
Indeed, we agree that the conclusion we draw was too broad. Therefore, we changed the part of the discussion on the effectiveness of universal depression prevention. In our discussion, we have tried to create a nuanced picture regarding the effectiveness of universal depression prevention, since the results are not conclusive. Concerning the review by Merry et al. (2011) we carefully reviewed all the articles included as universal depression prevention. We came to the conclusion that not all studies included can be regarded as ‘truly’ universal, since they did not include the majority of the addressed population or directed their intervention to a risk group. This is in contradiction with their own definition of universal prevention, since Merry et al. (2011) pointed out that universal prevention should be directed at an entire population regardless of risk status. By saying this, we do not argue that universal depression prevention is never effective. We think that it depends upon specific aspects of the program, since we also point out in our discussion that some universal programs do prevent depressive symptoms in adolescents.

See: p.17 - 18 Discussion, line nr. 422 – 440.

Minor Essential Revisions:
1. In your discussion of the mechanisms of change in CBT in the background, please make it clearer as to the population reviewed and intervention type.

Reply:
We hope that by adding information on the study population and intervention type, the discussion of the mechanisms of change in the Background section is more clear.

See: p.5 Background, line nr. 110 – 124.

2. Please revise your manuscript for spelling and grammar. At present, this makes the paper difficult to follow and disjointed in places.

Reply:
We have sent the revised manuscript to a editing service and followed all suggestions. We hope by doing this the paper is more easy to read.
3. Please provide more detail regarding what your sample size calculations are based on? What was the estimated ICC and how did this affect the calculations?

Reply:

The present study on depression prevention was the first RCT that our research group conducted into the effectiveness of a depression prevention program. Therefore, we based our sample size calculations and the correction factor for clustering in schools on RCT’s that had been executed before. In those trials, the correction factor used to account for the clustering in schools was 1.3-1.4 (Malmberg et al., 2014). Hence, the power calculation was performed as follows. For a dichotomous outcome: Cohen’s d = .20 en F = .10, this resulted in 788 participants. Correcting for 20% attrition over time (factor 1.2), and clustering in schools (factor 1.4), the sample would consist of: 788 x 1.2 x 1.4 = 1324 in total (662 per condition).

However, there are other methods to adjust the sample size for clustering. For example, Kerry and Bland (1998) describe that, in order to correct for clustering, the sample size should be multiplied by 1 + (m-1) * ICC, in which m = the number of participants per cluster. In previous research it was found that the average ICC for classrooms was .02 - .03 (Calear, Christensen, Mackinnon, Griffiths, & O’Kearney, 2009). To account for the clustering in classrooms according to this formula, we should have use a correction factor of: 1 + (26-1)*.025 = 1.63 (since our average classroom size was 26). This would result in a sample size of 788 * 1.2 * 1.63 =1541. This would result in a slightly higher sample size compared to the current sample.

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

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

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests: I declare that I have no competing interests.

Many thanks again for the valuable remarks of the reviewers. We believe that by addressing these points we have very much improved the readability of the manuscript. We hope that we have replied to the comments of the reviewers to the satisfaction of both reviewers and the editor.

Yours sincerely,

Yuli Tak
References:


