Reviewer's report

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

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Reviewer: Patrick Pössel

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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.
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.

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.

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.

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.