Reviewer’s report

Title: The effectiveness of an anti-stigma intervention in a basic police officer training programme. A controlled study

Version: 1 Date: 16 February 2014

Reviewer: Beate M. Schulze

Reviewer’s report:

Authors have made considerable effort to address both reviewers’ suggestions and clarify open questions regarding design and analysis. As a result, the manuscript has been further improved and, in my eyes, warrants publication as it stands at this stage.

A small number of additional changes would further add to the quality of the paper. They are inserted in bold print and are all discretionary revisions that the authors can be trusted to make.

The major compulsory revisions were successfully addressed as follows:

1) The intervention is now described in more detail and misleading phrasing has been removed. The present description (pp. 7-8) now gives a transparent account. This being said, the new section might still benefit from language editing to enhance the clarity of the description.

2) The rationale for suggesting that contact between the police and people with mental illness has increased in the era of de-institutionalization is now becoming clearer, particularly from the response letter. References from the grey literature (conference talks, etc.) were added to the manuscript. In addition, the manuscript would benefit if authors could specify, as in their response letter, that the “widespread notion” (p. 3) was predominantly voiced from within police and judicial circles based on their daily experience.

3) Authors now also present effect sizes on attitude changes from baseline to follow-up. The additional two columns in Table 2 are particularly useful in this regard. An additional minor discretionary revision would be to change the order of the two new columns in Table 2 in line with the chronology of the research process, i.e. move the Pre-Post effect sizes one position to the left. Further, two cells presenting Pre-Post effect sizes are empty (Community Ideology dimension of the CAMI, Q1 of the RIBS). Please explain. Also, while the further increase in effect size for the total attitude score after the post-intervention measurement would certainly be desirable, it is not clear by which mechanisms this additional intervention benefit could have come about in the absence of additional interventions. Please advance a number of possible explanations for this finding as part of the discussion.

4) Additional separate ANOVAs were performed which showed no significant interaction with the time-group analyses, demonstrating that the age difference between intervention and comparison group did not affect intervention outcomes.
Minor essential revisions were also adequately addressed:

1) The exact hours of the intervention content provided are now given (Intervention section on p. 7).

2) The argument why authors decided against a follow-up survey among the comparison group, i.e. that no changes in attitudes were found from baseline to post-intervention among controls, is plausible – both on ethical and economic grounds. Had they included this explanation now added on page 7 (end of first paragraph) in the original version, I would not have raised this issue since I think there should not be an automatism in demanding a standard purely on the basis that it is common scientific practice, particularly in in-vivo intervention research as compared to lab experiments.

3) Authors now mention the proportion of participants and controls with (rather than without) previous experience of mental illness. They also give the range of contexts in which they have come across the topic. This renders the relatively high proportion of those with previous contact (72% or 68%, respectively) more plausible. Finally, the new sentence added at the end of the second paragraph on p. 11 also shows that intervention and control group were comparable with regard to their previous experience with mental health problems.

4) The stringency of the paper’s argument certainly benefits from authors’ decision to remove reference to their equally interesting population survey from the current manuscript.

Overall, the study presented makes an important addition to stigma intervention research and warrants publication as it stands.

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:

I declare that I have no competing interests.