Author's response to reviews

Title: Queer Quit: A pilot study of a smoking cessation programme tailored to gay men

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Version: 4 Date: 23 January 2014

Author's response to reviews: see over
Dear Reviewers

Thank you very much for your critical review of our manuscript. Your feedback has helped us improve our manuscript substantially. Below you can find our point-by-point responses. We are looking forward to hearing from you.

Sincerely,
Maria Dickson-Spillmann

Reviewer 1

Major Compulsory Revisions

The authors just mention that the participants were given a letter for physician so that a medication prescription could be done. The authors should explain more explicitly how many participants actually received NRT or other medication and how much they used it to assist cessation.

This is indeed an interesting issue. We now present the numbers of participants using different types of NRT in sessions 3 and 7 (use of NRT was not assessed at the follow-up) : « In session 3, 28 of 59 (47.5%) participants reported using NRT. Varenicline tablets were the predominant therapy (used by 11 participants), followed by 2mg gum (5), 4mg gum (3) and 16h patch (3). In session 7, 18 of 47 (38.3%) participants used NRT, tablets still being the most frequently used therapy (9 participants), followed by the 2mg gum (4).”

In the Discussion, we repeat the result on the decreasing number of participants using NRT between quit day and the last session. We however decided not to interpret the observed reduction in NRT as it depends on the type of medication whether a reduction within four weeks is recommended or not. Only few participants reported the dose of NRT used, thus we did not evaluate this aspect.

How did the authors assess alcohol consumption and changes in it?

Alcohol consumption was assessed as part of the EuropASI. We assessed frequency of consumption in the past 30 days and the number of standard drinks. For this paper, only frequency was evaluated via GEE (time as the predictor and frequency of alcohol consumption as the dependent variable). We have added related statements in « abstract », « measures and instruments » as well as in « data preparation and analyses » : ...

- Abstract : « frequency » of alcohol and drug use
- Measures and instruments : Data on mental and physical health and 30-day point prevalence of drug and alcohol use were collected in sessions 1 and 7 and at the six month follow-up using the German short version of the Beck Depression Inventory (BDI-V) [12, 13], the Beck Anxiety Inventory (BAI) [14, 15], the Short-Form Health Survey 12 (SF-12) [16, 17], and a shortened version of the European Addiction Severity Index [18].
- Data preparation and analyses: Frequencies of alcohol and drug use were coded into different categories ranging from “never” to “3 or more times a day”.
**Minor Essential Revisions**

**RESULTS**

In the following sentence there are two errors. First, ‘measurement effect’ should be called ‘time effect’ and p=.00 should be marked as p<.001.

“According to the GEE analyses, “measurement point” had a significant impact on abstinence rates (OR 2.01, CI 1.60 - 2.52, p = .00).”

*In line with the other reviewers’ request, we have renamed « measurement point » to « time ». Throughout the paper, however, we report precise p values (except to summarize several analyses, where e.g. p<.05 was used). Thus, we have not modified the marking of the p value.*

**Reviewer 2**

**Minor Essential Revisions**

1. This sentence is confusing: "One element of particular importance appeared to be that an intervention for gay men would encourage recreational activities in a non-smoking environment, since smoking and going out to gay venues belonged together and smoking was perceived as a bonding element in the gay community" (Background paragraph 3). As far as I can tell, the quoted sentence is getting at the idea that past studies have suggested that smoking is perceived as a bonding activity in gay communities, so a successful intervention for gay men should reassure them that recreational activities are still possible without smoking. Regardless of whether I interpreted the sentence correctly, I think it would benefit from rephrasing.

   *We have rephrased the paragraph as follows :*

   Swiss gay smokers expressed widespread intentions to quit smoking and a high interest in a culturally specific smoking cessation programme [9]. The smokers in this study reported that within the gay community, smoking and going out to gay venues ‘belonged’ together. The participants feared that quitting smoking, without being provided appropriate support, would mean having to abstain from the gay scene and possibly losing community membership. Thus, the participants considered it important that any successful programme help to build a non-smoking social network that would support them beyond the end of the programme.

2. Depression and anxiety are introduced as "mental disorders" (Background paragraph 4) but later assessed on scales. I would suggest referring to them as "threats to mental health" or choosing some other phrase that indicates that they are being treated as characteristics varying continuously in the population rather than discrete diagnostic categories.

   *We now refer to “risk factors for decreased mental health”.*

3. The gender of the facilitators should be mentioned in the method section (paragraph 1 of the "Setting" section).

   *We have added this information in the respective paragraph.*
4. It is not clear why the authors calculated sums and means for the BDI and the BAI, as stated in the first paragraph of "Data preparation and analysis." They do not specify whether they use sum scores or mean scores in subsequent analysis procedures. The mean scores seem more appropriate given that the authors permit up to two missing values on each scale. Perhaps the note about computing sums could be removed, or the purpose in doing so could be clarified.

We understand that the reasons for calculating sums as well as means might have been unclear. The reason for calculating sums was to be in line with clinical research where sums are usually reported, and to be able to eventually compare depression and anxiety values to other samples (whereby only depression, but no anxiety values, were actually available for a representative German male sample). The reason for calculating means was as suspected by the reviewer— in order to allow for missing values. We have rephrased the paragraph in the following way and hope it is now clearer: In accordance with clinical research, where BDI and BAI scores are frequently reported in terms of sum scores, for our sample description we also calculated sum scores for these measures. For the analysis of BDI and BAI as secondary outcomes of the programme, however, we used the mean BDI and BAI values of those participants with no more than two (10%) missings.

5. The results section entitled "Participants' feedback and interviews with facilitators" groups open-ended feedback into categories (e.g., "group dynamics") without explaining the grouping system. Were there subjective coders, for example? If the authors feel that these categories of responses were important, the system of categorization should be clarified. If not, perhaps they are not worth reporting at all. It is worth noting that the discussion does not refer to any of the categories of responses, suggesting that the authors may not view them as essential.

At the end of the section “data preparation and analysis”, we explain how we evaluated the open-ended (qualitative) questions. We have added the word suggested (“Open-ended, qualitative feedback regarding the programme was coded into different categories suggested by one rater; this categorisation was then reviewed by another rater. In case of disagreement, the raters discussed both alternatives until they reached a consensus.”)

The findings of the qualitative responses are mentioned in the discussion, although not extensively: “Qualitative remarks, however, showed that many participants appreciated the atmosphere of open discussion at the sessions. Probably such an atmosphere more easily evolved through the match between programme contents and facilitators’ and participants’ sexual orientation”.

To give the qualitative findings higher priority in the discussion, we have now added a sentence at the end of the first paragraph: “The programme was well accepted by both the facilitators and the participants. Qualitative responses showed that the participants particularly appreciated the group dynamics and the group experience of the course. This finding underlines the importance for our participants to experience smoking cessation together with other gay men, which is a desire expressed in the context of a previous study [9].”

6. I feel that the authors go beyond the data in suggesting that the program could offer particular benefits for gay men with HIV (Discussion paragraph 6). They present no evidence that any of their participants had HIV or that their intervention had characteristics that would appeal to people with HIV. I believe
that the authors meant only to imply that any program that helps people quit smoking could have unique health advantages for people with HIV, but their phrasing implies a stronger stance about the unique benefits of their program. 

We agree with this point raised by the reviewer. We have modified the introduction to this paragraph to suggest that particular research attention should be given to smokers with HIV attending the programme. We no longer suggest that there might be particular benefits for these individuals. The paragraph now reads:

“Future research could focus on evaluating the programme in homosexuals with HIV. Due to the double physical burden, smokers with HIV are at enhanced risk for pulmonary, cardiovascular and bone diseases, as well as malignancies and generally lowered immune responses [30]. In addition, smoking is associated with lower adherence to highly active antiretroviral therapy [31]. Thus, promoting smoking cessation in HIV-infected smokers is a particular concern.”

7. I also feel it is important to mention that, due to the lack of a control condition, this study cannot rule out the possibility that these men (selected specifically based on a pre-existing strong intention to quit) would have stopped smoking on their own without the program.

We agree with this. We have added a respective statement in the limitations: “Because of the absence of a control condition, we cannot assess the influence of pre-existing intentions to quit smoking on our participants, which were possibly stronger than those of their peers who did not attend the programme and, as such, could have enhanced participants’ success regardless of program participation.”

Discretionary Revisions

8. The GEE model evaluates all three timepoints. A stronger test of the long-term effect of the program might be to fit a model comparing the baseline to the 6-month followup, ignoring any improvements observed at the end of treatment.

Based on the reviewers’ input, we have considered alternative ways of analysing the main outcome. According to the reviewers’ suggestion we could theoretically conduct a X² test comparing abstinence rates at baseline and follow-up (leaving out end of treatment); however, as all participants were smokers at baseline, this test cannot be conducted. Another option would be to simply leave out any tests of significance as the results (0% abstinence at baseline, 66% at end of treatment, nearly 30% at follow-up) speak for themselves. We have, however, finally decided to leave the analysis and results as they were, including the GEE of all three time points.

9. Are there any past studies of gay men going through smoking cessation programs not specifically designed for gay men? Such studies might provide especially useful comparisons in the discussion section.

We have conducted a literature research to answer this question and were able to identify one such study (Covey et al, 2009). In the discussion, we have added the following statements:

“A previous study found that cessation rates were nearly identical between homosexual (59%) and heterosexual (57%) male participants [28] at the last session of a non-tailored smoking cessation programme; rates comparable to those we observed. Follow-up results were not reported for that study. Thus, the effect of tailoring the program specifically to the homosexual population remains unknown.”
Quality of written English: Needs some language corrections before being Published

We have sent the manuscript to a language correction service prior to submitting the revised manuscript; please see the certificate.