Author’s response to reviews

Title: Driving Force of Condomless Sex after Online Intervention among Chinese Men Who Have Sex with Men

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Author’s response to reviews:

Dear Dr. Eva Szunyogova

Thank you for your helpful comments on this review. We have taken these comments and used them to make the review stronger. Find below point-by-point responses, a clean version, and a tracked changes version of the manuscript.

Thank you for your consideration.

Sincerely,
Response to the comments of the reviewer #1:

Reviewer reports:

Bo Qu, M.D., PhD (Reviewer 1): I very much enjoyed reading this manuscript. It is great to see more research coming out on the high-risk behavior among MSM. I have mostly minor feedback (below).

Comment 1. Consider referencing MSM as percentage of total population - very small portion of the population accounting for such a large portion of HIV incidence makes for a strong point here.

Response to comment 1. We agree with the reviewer. We have changed lines 56-60 in the original manuscript into “It is estimated that 1 out of 70 men could be MSM. If the current HIV epidemic in MSM were not contained, the overall HIV prevalence would increase from 9.2% in 2016 to 12.6% in 2020 and 16.2 in 2025.”

Comment 2. I may suggest that in the background, some literature review on the associated factors of condomless sex after intervention should be added.

Response to comment 2. We agree with the reviewer. We have added “However, online interventions still facing many challenges in behavior changing after the intervention, like limited breadth and depth of social media interaction, low “dosage” of the intervention, limited lasting long-term effectiveness, etc.” in the revised manuscript.

3. Please add the statistics (t-value, chi-square value …) in the table 2.

Response to comment 3. Per the comment of the reviewer, Chi-square test and t-test has been added to table 2.
4. In this study, multivariable logistic regressions were used to further identify the potential factors associated with condomless sex. It may be confused that why only the factors in the table 3 were included in the regression. The identity of students and sexual identity were also the associated factor as shown in table 2. Please add all the significant statistically factors in table 2 into the regression model.

Response to comment 4. Per the comment of the reviewer, these two factors have been added into the table 3.

5. The sample is largely comprised of people of younger age, limiting the generalisability of the findings.

Response to comment 5. We agree with the reviewer. We have added the concern of younger age of the participants into the limitation section at line 241-242.

6. The authors upload the links of original two videos in the manuscript. Unfortunately, I can only get access to the first video. But I can't get the intervention information from the video. Please check that.

Response to comment 6. We have re-uploaded the video. The crowdsourced video is at: https://www.youtube.com/watch?v=Ib_7u5VH1Ck and the social marketing video is at: https://www.youtube.com/watch?v=WNtoI1MdB0c. The links of the two videos have been updated at line 109-110.

Response to the comments of the reviewer #2:

Yuhua Ruan (Reviewer 2):
1. Due to 791 (67.4% of baseline) participants completed the three-month follow-up, it needs to compare baseline characteristics between who completed and who did not complete the three-month follow-up.

Response to comment 1. We agree with the reviewer. We have added the comparison of baseline characteristics between participants who completed and who did not in the table of supplement A.

2. It should add data about reasons for lost to follow-up.

Response to comment 2. We agree with the reviewer. Since we did not collect the data on reasons for lost to follow-up, we have added this concern as one of the limitations at line 238.

Response to the comments of the reviewer #3:

LaRon Nelson (Reviewer 3):

The scientific premise of the study appears to be flawed. The secondary analysis aims to test what factors may attenuate or optimize the effectiveness of two online HIV risk-reduction interventions for Chinese MSM. Here are some key points that informed my assessment that the manuscript should be rejected:

1. It is not established in this paper that either of the interventions were effective. The paper only presents evidence that there was no difference between the crowdsourced video and social marketing video on reducing condomless anal sex. This is, not the same as establishing that both interventions are effective.

Response to comment 1. Thanks for the concern of the reviewer. The original study is a non-inferiority trial; a trial specifically tests whether one intervention is non-inferiority to another one. Given the social marketing approach was considered to be the standard method for developing and implementing health communication interventions, it is reasonable to compare a
new intervention (crowdsourced intervention video) to an existing intervention1-3. This method
had been widely used in previous studies4.

In addition, this manuscript is not to compare these two interventions, the intervention
comparison has been reported in another paper5. This paper is only aimed to evaluated factors
associated with condomless sex after the intervention.

To clarify this, we revised the last paragraph of the background section into “We have conducted
a non-inferiority randomized controlled trial using online video intervention to promote condom
use among Chinese MSM. The two video interventions in the study was found that could both
effectively promote condom use in the follow-up period.” at line 77-80.

2. The paper presents that condomless sex in the interventions decreased to 52.1% and 49.6% in
each of the interventions. It is not clear whether these were statically significant reductions in
condomless sex from baseline.

Response to comment 2. Thanks for the concern of the reviewer. We have further clarified this in
the revised manuscript. At baseline, everyone engaged in condomless sex in the last three months
(100%), it is one of the inclusion criteria for this non-inferiority study. The condomless sex had
reduced from 100% to 52.1% (t=-8.46, p<0.01) and 49.6% (t=-8.78, p<0.01) in the crowdsourced
and social-marketing arms, respectively. We have revised the manuscript accordingly at line
104-105.

3. The finding that people who were community engagement in sexual health at baseline was
associated with reductions in condomless anal sex at follow-up seems to suggest that the
intervention may not be responsible for the effect on condomless anal sex.

Response to comment 3. We understand the reviewer’s concern. To deal with this problem, we
have included types of intervention in the logistic regression in table 3. The purpose of
conducting this regression is to identify factors associated with reducing condomless sex besides
the video interventions. The association between higher level of community engagement in
sexual health and reduction in condomless sex suggests that people who had more community
engagement could be more likely to change their behavior of condomless sex besides the
intervention. In other words, community engagement could facilitate the effects of reducing
condomless sex with the video interventions.
4. The community engagement tool is described as a scale. It will be important to present specific information about its validation with Chinese MSM and its basic psychometric properties.

Response to comment 4. We agree with the reviewer. This scale has been validated previously by our study group. We have further clarified this in the revised manuscript and cited the validation study at line 124 of the revised manuscript.

5. A further important flaw in the design is that there is not a comparison group to determine whether the reductions in condomless anal sex are due to the online interventions. This cannot be overlooked given that the authors have proposed this paper as investigating the impact of specific factors on intervention effectiveness, without first establishing that the interventions are effective.

Response to comment 5. As we have responded to comment 1, the original study was designed as noninferiority randomized control trial. Social marketing has been used as a standard method for developing and implementing health communication interventions. Crowdsourcing is an innovative approach to develop health intervention material. It is reasonable to compare the new intervention (crowdsourcing) to an existing standard intervention (social-marketing). The intervention comparison has been reported in another paper.

6. Table 1 included transgender as a sexual identity. It is a gender category, not a sexual identity. It would be acceptable for their study to have included transmen who have sex with men; but, this conflicts with the inclusion criteria which indicates that participants were biologically male at birth. This leads me to conclude that perhaps transwomen were in the study sample. Either way, this further limits my confidence in the study methods, results and interpretation.

Response to comment 6. We agreed with the reviewer that this category should be a gender category instead of sexual identity. We revised the “sexual identity” into “gender identity” in table 1. The categorizes of the gender identity are: gay, bisexual, heterosexual, and unsure. Bisexual, heterosexual, and unsure were grouped as “others”. In the study, we included participants who were born biologically male and ever had sex with men, no matter what their gender identities were.
References:


