Reviewer's report

**Title:** An Exploratory Randomised Controlled Trial of a Premises-Level Intervention to Reduce Alcohol-Related Harm Including Violence in the United Kingdom

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**Reviewer:** Stella Resko

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This article describes an exploratory pilot study of a risk assessment intervention with alcohol serving establishments (e.g. bars) in the UK. The goal of the intervention is to reduce violence in and near these establishments. This study builds upon previous research and the authors, in general, provide good arguments for the need for this particular study. The primarily quantitative study used multiple data sources (e.g. secondary analysis of police data, primary data collection with the establishments and patrons). The use of the multilevel model with the street survey data and the Andersen-Gill (1982) cox proportional hazards model with the police violence data seem appropriate given the data. The authors also mention the use of qualitative focus-group data as part of this study, but the quantitative results appear to be the focus of this manuscript.

Although this paper addresses an interesting and important topic, there are some issues that should be addressed prior to publication. The paper needs some restructuring and rephrasing so that the ideas presented are more organized, concise, and clearer.

Specific comments/suggestions are listed below under each section heading and are referred by paragraph number within each section. Some suggestions are major and the rest are discretionary or minor revisions.

**Major Revisions**

1. A double-blind study is an experimental procedure where neither the participants of study nor the persons administering the study know the critical aspects of the experiment. The authors of this paper describe their study as double-blind but it does not appear that the participants were unaware of their randomization to either the experimental or control group. Some of the comments in the results, for example, by the establishments suggests that they were aware that they were receiving an intervention (e.g. Implementation Barriers section: “I’d like to see how we stack up to see if anything has changed with implements that we’ve put in”). Based on the authors description it appears that the research staff were blinded but the participants were not.

2. In paragraph three of the introductory section there is very little in-text description of the empirical and theoretical literature that supports the type of
intervention that the authors have developed and are testing. This needs to be described more in this paper. This description may be somewhat brief given that the authors have already published a paper describing the intervention and study methods (prior to study completion). The authors do however need to provide a reasonable overview of these topics so this paper can be read as a stand-alone document.

3. There are a number of issues related to the organization of this paper, particularly within the methods and results sections, that need to be improved. In the current format, the methods section is difficult for a reader who is unfamiliar with this study to follow. The authors for example, talk about individual level data collection (e.g. the breathalyzer surveys and FAST survey) before they explain that they collected any individual level-data from patrons outside the establishments. Moving up the content of the data collection to the beginning of the methods section would be very helpful for the reader. Moving the sections on statistical analyses and sample size to a position earlier in the methods would be helpful. The descriptions of the FAST screening could be improved to explain what this questionnaire actually measures. The current description explains how the FAST assessment is widely used and valid but does not specify what is being assessed. When describing the police crime data a specific time period is mentioned, but it is not clear to the reader at that point what the general time frame for the study was.

4. The authors should consider changing the order of the sub-sections within the methods section in order to enhance readability. It might also make sense to combine some of the related sub-sections (e.g. the matching and recruitment of establishments; the randomization and blinding). The description of how premises were recruited could also be made more clear and could include a description of how many establishments refused to participate in the study. The role of the auditors should also be more clearly described in the methods section. Readers outside the UK would also find a brief description of what an auditor is to be helpful.

5. One aspect of the data collection that I had a question about is whether the surveyors were completing the ratings of drunkenness after seeing the participant’s reading on the alcometer. If that is the case, the two scores are not independent and it may be problematic to conclude based on the current study that the ratings of drunkenness would suffice on their own. If that is not the case the authors should be more clear about this point.

6. Figure one is included in the appendices but not mentioned until far into the results section (under the process evaluation subheading). It would help if the authors integrate Figure one and the content from the process evaluation section earlier in the paper (e.g. the methods section).

7. This pilot study is complex and involved a number of different data sources. The focus group data receives very little attention in this paper and it may make the most sense to remove this content from this paper. In the current format there is not enough space to adequately describe the methods and results of this
aspect of the study.

8. Some of the content presented in the results section (e.g. the first couple sentences in the violence results) could be moved to the methods section.

9. The titles given to some of the subsections also could be improved. Much of the content in the statistical analysis section in the methods is information that is not typically included in such a section. In the results, the heading “missingness” also seems out of place with what is generally referred to when discussing missing data. The authors appear to be describing the participation and refusal rates not missing data (e.g missing values for particular variables). This information could also be presented either at the beginning of the results section or in the methods.

10. In the results the authors note 21% of participants experienced violence. There is no description of how violence was defined. This measure as well as the violence perpetration measure should be described in the methods section. Also, given the cross-sectional nature of the patron surveys it might behoove the authors to state there is a relationship between perpetration and victimization but not state that one predicts the other. The data was not collected in a sequential format so that they could determine whether victimization precedes perpetration or vice versa.

11. The analysis related to the cost-benefit analysis is a minor part of this paper and it might make sense to remove this section from the results since it is not mentioned earlier. In the discussion section, the authors could mention how this intervention and others like it may be attractive given their limited costs associated with implementation and the potential reduction in costs associated with violence.

12. More detail on how the establishments received feedback from the site-visits and whether they were able to have any input in the development of the action plan would be helpful. Did the establishments receive a written report only? What happened when the establishments did not agree with the plans? Was there any consideration to renaming the “audit visits” something more neutral or less threatening to the establishments (e.g. site visits). These issues are only briefly addressed in the Implementation Barriers and Fidelity Section of the results.

13. In the first paragraph of the discussion section, the second sentence is long and unclear particularly since the authors do not describe the nature of alcohol outlet licensing and oversight until this point in the paper. Keep in mind that readers outside the UK are not likely to be familiar with these regulations. It also was not clear whether the auditors that conducted the audit-visits have some official oversight role and are able to suspend an establishment’s license.

14. The language describing the results of the power analysis in paragraph 4 of the discussion section needs revised. The current wording states that a “12 month follow-up period across 517 premises would be required to raise this effect into significance...”. It might be more appropriate to reword this (e.g. to detect a
significant improvement).

15. In paragraph 4 of the discussion, the authors suggest the effectiveness of the intervention could be increased if the intervention receives police enforcement. Could such an intervention also decrease the motivation of the establishments to participate in such an intervention or be forthcoming during interviews given the higher potential risks (e.g. revocation of their license)?

16. I’m wondering about the use of the word failure throughout this paper. Did the authors choose to use this word because it is the language typically associated with the Andersen-Gill model or other cox proportional hazards models. Is there another word that might make more sense given the particular focus of this study? It comes across as a very negative term that includes a negative judgment of the establishments. The approach of this intervention seems more aligned with a criminal justice approach. I wonder if the authors/developers of the intervention considered whether another (e.g. motivational interviewing) would be more appealing to the establishments and be able to achieve better results.

Minor Essential Revisions

17. Acronyms should be defined when they first appear in the paper (e.g. NHS in paragraph 1 and DPS in paragraph 2 of the paper’s intro section, NTE in the methods section).

18. In paragraph 1, the comment “while alcohol is associated with young peoples’ risky sexual behavior” seems out of place given the focus of the rest of the text.

19. In paragraph 2, the wording could be improved. It might be helpful to consider dividing the first sentence into 2 parts (e.g. ending the first sentence after the word violence, cutting out the words “suggesting that” and beginning the second sentence with premises level. In the sentence that begins with Whilst, the sentence would read better if the word “whilst” was deleted.

20. In Figure 2 (the first box in the upper left corner), premises is misspelled.

21. In the Recruitment section (in the Methods), the authors could be clearer in describing the number of communities that are represented in their data (e.g. the total as well as the characteristics of these communities as large cities, towns etc.).

22. In the first sentence in the measures section (“The outcome measures used in this study…””) the authors state that measures were selected based on their appropriateness and objectivity. It might make sense to say they were selected based on their appropriateness and feasibility for the particular setting. Some scholars would challenge the designation of all of the selected measures (e.g the research staff’s ratings of drunkenness) as “objective”.

23. Is there a citation for the formula used to calculate estimated risk for each establishment (in the methods section)?

24. In the data collection section (of the methods), there is an extra period at the
end of the first sentence in the second paragraph.

25. In the last sentence of the second paragraph of the results violence section, the last part of the sentence does not make sense and needs edited (“…and thereafter a modest reduction post-intervention in the experimental group.”)

26. In the last sentence of the implementation barriers and results section there is a typo where the word stated is used and should be started.

27. In the second paragraph of the discussion section, the authors state “Street surveys are the only feasible mechanism…” The authors may want to remove the word “only” because while the street surveys are feasible, it may be overstating things to suggest they are the only feasible option. Similarly in the last sentence of the 5th paragraph in the discussion section, it may be an overstatement to say that “premises are potentially the only point at which interventions can be delivered to the majority of risky drinkers.” (note this sentence also has a typo—of is listed as or).

28. The wording of the third paragraph of the discussion does not fit with the wording of the other sections. It might

29. A typo is in the 5th paragraph of the discussion (see the acronym DPS).

30. In the second to last paragraph of the discussion, the sentence that begins “This in turn mediated…” could be clearer.

Discretionary Revisions

In the statistical analyses section the authors state that they “develop” the Andersen-Gill model. This is a minor wording issue, but it might be better to use another word (e.g. utilized, calculated) so that it does not sound like you developed the statistical test.

In the second paragraph of the Intervention revisions section, the word “overhead” is a little awkward and it might be better to use another word (e.g. inconvenience).

When describing the breathalyzer survey (under measures in the methods section), it would also help to add another sentence or two describe the street survey methods more clearly.

**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:** No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:

I declare that I have no competing interests.