Author's response to reviews

Title: Reasons for non-participation in a parental program concerning underage drinking: a mixed-method study

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Author's response to reviews:

23th October 2009

Dear Editor,

Thank you for considering our paper for publication in BMC Public Health. We are pleased to respond as far as possible to the reviewers’ comments and to submit a revised manuscript of our article, Reasons for non-participation in a parental program concerning underage drinking: a mixed-method study.

Besides the changes that the reviewer suggested, Editorial team required that the paper should be copyedited by a native English speaking colleague and that we should document the informed consent. The paper is now language checked and a sentence about the informed consent is added in the method part.

Best regards

Camilla Pettersson
Margareta Lindén-Boström
Charli Eriksson

Respond to reviewer’s report:

Reviewer: Sven Bremberg

The paper addresses a question of practical importance - participation in education programmes, in this case a parenting programme. It is scientifically sound. Yet, the paper would gain in readability if it is shorted with focus on the main findings.

- We agree with the reviewer that this article addresses a question of practical importance. How to increase participation is a central question for public health workers who implement parental programs. It is also important that people developing programs consider how a parental program should be designed to be
attractive to all kinds of parents. From a public health perspective, parental interventions should promote equity and social justice. This is a real challenge as some educational interventions have resulted in an increased health gap in the short run.

- We disagree to the comment about shortening the article. From our point of view, all parts of the paper are necessary to answer the three research questions that are addressed.

1) The Health Belief Model (HBM) is adequately used. References are mainly given to its use in parenting programmes. Yet, references to a wider range of educational programmes would be helpful since a common finding is the lacking predictive power of this model.

   - In this paper we use the HBM as a theoretical framework and our ambition is not to make a formal testing of the model. In the paper we make a new reference to meta-analysis of previous research not limited to participation in health programs. Moreover, we indicate the modest predictive value of the HBM for predicting health behaviour.

2) There are some unclear points.
   - Page 2, para 1. It is stated that a "quasi-experimental" method was used. In the methods part I did not find out the design of the experimental part.
     - We have rewritten the first sentence in the method.
   - Page 9, para 3, line 3, There seem to be more participants (7613) than eligible parents (895).
     - The sentence has been rewritten
   - Page 9, para 3, it is stated that "In total, 718 parents had answered one or several of the questionnaires.", Yet the inclusion criterion was at least two questionnaires answered.
     - The paragraph has been restructured.

Reviewer: Michael Ichiyama

Major Revisions

1) Page 4, 1st paragraph. The “Background” section needs more information. I would suggest a brief review of parent-based alcohol prevention programs (at least provide some examples from other studies). I would also include something about peer influences. There has been debate in the literature about the relative impact of parents vs. peers on adolescent behavior generally. Some mention of this seems warranted.
- We have added a brief review of parent-based alcohol prevention programs in the first paragraph.

- We added a sentence and a reference about peers influences on the development of adolescents substance use. We really think that it is an interesting aspect, but it is important for us to focus on parents and their possibility to prevent underage drinking.

2) First paragraph, page 11. Does any data exist on how many of the 13 “Strong and Clear” activities the participants and partial non-participants actually completed? I believe these groups were based whether they met inclusion criteria and the subjective assessment of whether they considered themselves “participants”. However, it would be interesting to know how many activities were actually attended among these groups. From my read of the manuscript it could well be the case that some who were partial non-participants actually completed more activities than some parents who considered themselves participants (please correct if this is an incorrect assumption). If this data does not exist at least provide some mention of this issue.

- An analysis of numbers of activities completed in the program has been accomplished and it showed that there were only small differences between participants and partial non-participants in numbers of activities completed in the program. We added a sentence about it under the heading Measures.

3) Page 23, last sentence. Recruitment and retention are important issues in parent-based interventions in light of the non-participation issue. The authors should describe the potential use of systematic follow-up contacts with parents to increase recruitment/participation rates. Phone contact after 1 week, then 2 weeks, etc. These are commonly used to increase recruitment rates in survey research (see text by Dillman). Also, the authors could discuss the use and/or viability of incentives to increase participation.

- We added an example from a previous study about how to increase participation in a parental program. We also mentioned that there are some culturally differences between Sweden and US when it comes to using incentives for participation.

4) Conclusions, page 25. Too short. Authors should sum up but also provide more of a discussion of future directions and how specifically the study findings can contribute to the design of future parental intervention projects.

- The conclusion has been rewritten.
Minor Revisions

1) Page 4, 1st paragraph. The authors state that parental participation in previous studies has been “generally low”. Could you provide some percentages or other statistics from these studies to give the reader some idea of what “generally low” is?
- Examples from previous studies are added.

2) The last sentence in the 1st paragraph on page 5 beginning “The literature gives support . . . “ needs to be rewritten. It is hard to follow.
- The sentence has been rewritten.

3) In the second paragraph on page 6, “self-administrated” should be “self-administered”.
- Changed

4) Page 20, 2nd paragraph. “. . . high as well as low” reads better as “. . . low as well as high”
- Changed

5) Page 22, last sentence. Sentence starting with “Parents who did not think . . .” is confusing and should be rewritten.
- The sentence has been rewritten.

6) Page 16, 2nd paragraph. Discussion of differences between schools are mentioned as a possible explanatory factor regarding the results. I think you should speculate on what sort of differences you are referring to.
- We discuss the differences on page 21, paragraph 3.

7) Page 20, 3rd paragraph. Authors mention that school with highest participation rate was used as the reference for the logistic regression. This was not made clear in the Method/Analysis section and should be. In the same paragraph you note that “contextual factors” may account for some of the findings. You should provide examples of such contextual factors for the reader.
- Information about why School 1 was used as the reference school is added in table 1.
- An example of a contextual factor that could have affected the participation
rates is given and we also have made the sentence about contextual factors more clear.

Discretionary Revisions

1) Here is a structural suggestion that would begin on page 4, 2nd paragraph, that might make the manuscript easier for the reader to follow. At the beginning of the second paragraph I would insert a heading something like “Factors influencing participation in parental support programs”. There also needs to be a transition sentence at the end of the second paragraph to lead into the following subsections. You then go into these factors, “Socio-demographic factors”, “Psychological and behavioral factors”, etc. These should become subheadings. Then at the bottom of page 6, the subheading “Towards a theory-driven analysis . . . “ should become a heading.

- We totally agree and have restructured the introduction as the reviewer suggested.

2) At the bottom of page 7 to the top of page 8. This paragraph moves abruptly from the theoretical to the analytical. I know they are linked but in the middle of the paragraph you describe how socio-demographic factors are “entered first”. I think the reader would benefit if you briefly delineated the regression model strategy before describing the analytic procedure even if clearly linked to the theory. I think the sentence beginning with “Therefore, sociodemographic . . . “ should be a separate paragraph.

- We deleted the sentence “Therefore, sociodemographic . . . “. Figure 1 gives enough information about the analytic procedure.

3) The literature on parental monitoring and parental relationship quality should also be noted in the Intro especially since they come out in the content analysis (see page 17).

- We added a sentence in the first paragraph in the background about the importance of a good relationship within the family and parental knowledge about the child’s everyday life.

Quality of written English: Needs some language corrections before being Published

- The article has been copy-edited by a native English speaking colleague.