PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Understanding behavioral changes through community based participatory research to promote oral health in socially disadvantaged neighborhoods in Southern Sweden
AUTHORS	Ramji, Rathi; Carlson, Elisabeth; Brogårdh-Roth, Susanne; Olofsson, Anna; Kottorp, Anders; Rämgård, Margareta

VERSION 1 – REVIEW

REVIEWER	Lance T. Vernon, DMD, MPH Cleveland Wade Park VA Medical Center USA
REVIEW RETURNED	08-Dec-2019

GENERAL COMMENTS	1) Overall, the topic is interesting and the research team has uncovered unexpected findings that reinforce the importance of this kind of qualitative investigation/intervention. The approach is novel and fresh, a welcome change from more traditional approaches. 2) The introduction is well written, but as bit long. It is good and it does set the stage. Could these main points be covered with fewer words? 3) The qualitative methods used is not my area of expertise, so another reviewer should comment on this area. It would be nice to be more transparent and see a table or graphic to show the percent of time certain themes were expressed, or when they emerged over the course of the study. 4) The quotes in the results section are excellent. 5) Also, a diagram that describes the time frame of the study, the number of participants and number of meetings would be helpful. 6) The word count, 7370, far exceeds the suggested 4000 word max. Could some details be included in a supplemental section? 7) In the Abstract, the first sentence of the Results seems to lack face-value clarity, and the second sentence appears to imply that one main theme may not have not been well defined--which suggests ambivalence by the authors, or a lack of clarity. For example, “family dynamics,” while brief, seems imprecise—could one or two words be added to ground these concepts for greater clarity to the reader (without needing to read the full text)? For example, “Passive stances and dysfunctional family

	assumptions/dynamics"—(or a phrase describing the effects of patriarchy)—perhaps this implies a negative judgement, but hopefully you get the idea. 8) The discussion has some areas that need to be addressed. a. There are many areas that read, for example, "…this led to increased empowerment" (not an exact quote). As empowerment was not measured quantitatively, such comments can not be made with certainty. Yes, I agree, it appears that empowerment was increased, but, in this context, behavior change is a "black box"--perhaps assertiveness and questioning patriarchal stances led to the change. It would be fair to say that, "participants reported and increase in X, Y or Z", or that "there appeared to be an increase in empowerment". But at present, some conclusions are stated with too much certainty given what was done, measured and observed. Another example (p 22), "The interaction between individuals in the group [appeared to] exert a strong influence on behaviors…" In the Conclusion (p 26), the phrase "…empowers individuals…" could read "..appeared to empower individuals…" b. Consider making a case for empowerment in the conclusion and giving it its own paragraph pulling off of data. c. The writing in the Discussion is wordy and indirect; there are many run-on sentences before and after citations #62, #63 and at the top of page 23 ("The stages leading to…") As a reader, I lost interest. This needs to be re-written and made more concise, precise and pithy. Please remove the redundancy throughout the text especially in the Discussion. d. Mention TePe and why this person was important and did not bias the study in the methods, not at the end of the text—it call more attention to itself there. e. The lack of quantitative assessment is a major limitation and should be listed first; order limitations in terms of their relative or overall importance. 9) Please document how long each meeting was. This is important when comparing these data to traditional and less time-intensive dental visits. 10) I suggest having people not associated with the study proof-read the manuscript to identify areas that are vague, unclear or repetitive.

REVIEWER	Anne Marie Coll, Wayne Richards, Teresa Filipponi Faculty of Life Science and Education, University of South Wales, United Kingdom
REVIEW RETURNED	04-Jan-2020

GENERAL COMMENTS	This paper is an innovative, important and positive paper with an essential message to health promoters regarding their interactions with disadvantaged groups. This message is that communications with these groups need to be contextualised and sensitive to the culture of the population in need rather than those concerned with the need. From the perspective of oral health, in westernised societies it is known that dental caries is preventable however there is a section of the community with relatively high levels of disease, the disadvantaged.

	Within the text of the paper even though there is evidence that behaviour changes were established, it seems that the behaviour changes were focused towards the amount of sugar consumed and thus the 'Fight Against Sugar'p27 line 17. Little evidence is presented to show that there was an understanding that the frequent consumption of lower levels of sugar could cause caries. Whether frequency of consumption of sugar was a feature of the discussions within the groups is unclear. However, there is evidence in the introduction that the frequency of consumption may not have been a focus of discussion p4 line36: "bacterial action leading to tooth destruction" Should it read: "bacterial action leading to the demineralization of tooth enamel. If demineralisation of tooth enamel through frequent intakes of sugar is allowed, then demineralisation exceeds the remineralisation of enamel with resultant destruction. Enamel remineralisation occurs within a sugar free mouth." Even though the research is very much and rightly focused on the population in need, there is some responsibility with the 'leaders/experts' to direct in an appropriate direction. The concluding section could have addressed this. Indeed, only time will tell whether lower levels of caries will be observed in the study population. From the perspective of public health, in the introduction the authors talk about disadvantaged communities; however, it became clear that the cohort of participants were migrants. The authors do not clarify the nature of the population engaged. They speak Arabic however the country of origin is not known; how long have they lived in Sweden? Are they refugees? It is not clear if the children were born in Sweden or have migrated with the families. The role of the women, diet, healthy eating and sugar consumption are reported and discussed in depth; however, oral health in the data presented is reported only on two occasions (one quotation by an 11 year old child and by a mother). The link with oral health per se is weak. Furthermore, reference is made to 'dialogue-based teachings' facilitated by the experts however, it is not clear what these were. It would have been beneficial had more detail been provided here as well as how these empowered the participants, especially the women. It would have been interesting to understand/discuss if the unhealthy behaviours were from their cultural background (e.g. drinking tea) or learnt from moving to a different country (e.g. drinking juice). From a methodological perspective, Community based participatory research (CBPR) was an appropriate choice of research design for this study which is not only grounded in the lived experiences of the community but endeavours to engage members of 'underserved' communities in effective interaction based on principles of co-learning and empowerment. This study has made a worthy attempt to achieve this. The research question was appropriate and the sample size was acceptable. The method of data collection was also appropriate based on 9 multistage focus groups over a six month period.

	The role of the 'health promoter' needs to be made more explicit as it is not clear what this role involved other than in the recruitment of participants and translation from Swedish into Arabic. One wonders whether this was an appropriate title in the first place. The data analysis is based on rapid identification of themes from audio-recordings (RITA) which is an appropriate method which also includes method and investigator triangulation and is in accordance with the Lincoln and Guba's (1989) framework of quality criteria of credibility, transferability, dependability and confirmability. Although ethical approval for the study is stated with voluntary, informed consent obtained, the details pertaining to confidentially and anonymity of the data are not discussed. It is also not clear how long the participants were given before informed consent was obtained. The limitations relating to selection bias and the potential for a conflict of interest by including a private oral health company are acknowledged and have been sufficiently justified by the authors. The transcript could be improved by proof-reading as there are some instances when the definite article is missing e.g. page 12 (line 10) and typographical errors e.g. page 15 (line 35). Some statements made in the introduction are not supported by citations e.g. page 5 (line 50). Data could also be included to support statements.

VERSION 1 – AUTHOR RESPONSE

Response to Reviewer 1

This paper is an innovative, important and positive paper with an essential message to health promoters regarding their interactions with disadvantaged groups. This message is that communications with these groups need to be contextualised and sensitive to the culture of the population in need rather than those concerned with the need. From the perspective of oral health, in westernised societies it is known that dental caries is preventable however there is a section of the community with relatively high levels of disease, the disadvantaged.
The authors would like to thank you for your supportive words and interesting comments.

Within the text of the paper even though there is evidence that behaviour changes were established, it seems that the behaviour changes were focused towards the amount of sugar consumed and thus the 'Fight Against Sugar'p27 line 17. Little evidence is presented to show that there was an understanding that the frequent consumption of lower levels of sugar could cause caries. Whether frequency of consumption of sugar was a feature of the discussions within the groups is unclear. However, there is evidence in the introduction that the frequency of consumption may not have been a focus of discussion p4 line36:

“bacterial action leading to tooth destruction”
Should it read:
“bacterial action leading to the demineralization of tooth enamel. If demineralisation of tooth enamel through frequent intakes of sugar is allowed, then demineralisation exceeds the remineralisation of enamel with resultant destruction. Enamel remineralisation occurs within a sugar free mouth.”

The authors are grateful for this suggestion. We have now rewritten the sentences on p. 3, lines 97-98.

Even though the research is very much and rightly focused on the population in need, there is some responsibility with the ‘leaders/experts’ to direct in an appropriate direction. The concluding section could have addressed this.

The authors do not dispute on the indispensable knowledge the experts have in this field. However, the current study has chosen a CBPR approach that asserts on the need to neutralize power relations and place the community members and other stakeholders including the experts and leaders in a neutral environment in an attempt to make the voices of the unheard, in this case the participants better heard. Furthermore, the community members and health promoters are raised in the Freirean principles that change can only be brought about by those suffering with problems themselves. The role of the experts as suggested here is to trust in the abilities of the citizens living in disadvantaged conditions and supporting them with the tools that the participants themselves identify as necessary to make change.

Indeed, only time will tell whether lower levels of caries will be observed in the study population.

The authors agree to this without hesitance since we have not performed clinical or other quantitative assessments to complement the qualitative evaluations.

From the perspective of public health, in the introduction the authors talk about disadvantaged communities; however, it became clear that the cohort of participants were migrants.

We have now clarified that migrants and ethnically different populations frequently live in disadvantaged neighborhoods in the introduction section on p. 2, line 71.

The authors do not clarify the nature of the population engaged. They speak Arabic however the country of origin is not known; how long have they lived in Sweden? Are they refugees? It is not clear if the children were born in Sweden or have migrated with the families.
The authors would like to clarify that we did not obtain any demographic information from the participants. However, through the discussions with them it was understood that most of the participants were from the Middle East with roots in Iraq, Iran, Lebanon or Syria. Furthermore, given that the children were fluent in Swedish and that they went to Swedish schools we understand that most of them have lived in Sweden for a few years and are not newly arrived immigrants, although we do not have data to support this. The study had no specific inclusion criteria and therefore included interested families from the specific neighborhood. We have presented this information on p. 9, lines 245-249

The role of the women, diet, healthy eating and sugar consumption are reported and discussed in depth; however, oral health in the data presented is reported only on two occasions (one quotation by an 11 year old child and by a mother). The link with oral

health per se is weak.

The authors agree that there is lack of direct evidence linking to oral health. However, we think that given that previous studies have highlighted the role of sugar and diet in dental caries prevention the objective of addressing the behavioral aspects was indeed to minimize caries incidence and development that in a way may contribute to improved oral health.

Furthermore, reference is made to ‘dialogue-based teachings’
facilitated by the experts however, it is not clear what these were.

During each meeting, the participants try to identify a common problem in the community, explore the problem further to identify resources and solutions while simultaneously implementing them to bring about transformation. As a first step in this process, the participants gained knowledge from experts like dieticians, nurses or dentists, in the form of a dialogue exchange. Some examples of the topics selected by the participants include discussions on sugar content in their routine diet and possible healthy alternatives to it (with a dietician). Pediatric nurses provided information regarding psychosocial support for behavioral change. The dental experts in this study were present during all occasions and added knowledge concerning oral hygiene, fluoride and the role of diet in relation to oral health. This information is included in the revised manuscript on p. 11, lines 290-299.

It would have been beneficial had more detail been provided here as well as how these empowered the participants, especially the women.

Freire states that the consequence of offering knowledge via dialogue as a tool enhances individuals control over self and their beliefs thereby leading to self-empowerment and such an empowerment may result in behavioral change. This is also further supported by Zimmerman’s empowerment theory that suggests that any action that triggers individuals perceived control over their life may bring about social change that may be facilitated by the process of increased empowerment. These discussions are included on p. 23, lines 559-562, as well as, p. 24, lines 597-600.
It would have been interesting to understand/discuss if
the unhealthy behaviours were from their cultural background (e.g. drinking tea) or learnt
from moving to a different country (e.g. drinking juice).

Thank you for these comments. However, we believe that the focus of this study was to understand change in the participating families’ dietary routines with the motivation to promote oral health. Therefore we refrained ourselves from any discussion that may stigmatize or demarcate a particular culture or tradition, rather assisted them through knowledge mobilization to make their own informed decision. Aside of this there is no evidence in the current study that drinking juice was related to moving to a new country.

From a methodological perspective, Community based participatory research (CBPR) was an appropriate choice of research design for this study which is not only grounded in the lived experiences of the community but endeavors to engage members of ‘underserved’ communities in effective interaction based on principles of co-learning and empowerment. This study has made a worthy attempt to achieve this. The research question was appropriate and the sample size was acceptable. The method of data collection was also appropriate based on 9 multistage focus groups over a six month period.

Thank you for the encouraging words.

The role of the ‘health promoter’ needs to be made more explicit as it is not clear what this role involved other than in the recruitment of participants and translation from Swedish into

Arabic. One wonders whether this was an appropriate title in the first place.

Health promoters are representatives from the community in consideration who primarily help the research team understand the cultural and contextual nuances that are specific to the community. Aside of this they also help with participant recruitment and language translations. We call them health promoters and not community health workers as often done in an international context since they differ in that they are educated in participatory action research methods and are groomed by the Freirean principles, which guide them in the health promotional work they support. This information was also presented in the manuscript on p. 7, lines 193-200.

Although ethical approval for the study is stated with voluntary, informed consent obtained, the details pertaining to confidentially and anonymity of the data are not discussed.

Thank you for bringing up this. Participants were ensured confidentiality at the time of data collection. In addition, participants were also informed that all results were to be presented abstracted and presented at a group level and no individual shall be identifiable through their expressions in neither reports nor scientific articles that emerge from this study. This information was explained verbally, as well as, included in the information letter that they received when they signed the informed consent. Considering the nature and design of the multistage focus group, it may be difficult to ascertain confidentiality however, the research team explained to the mothers concerning this and requested them to refrain from discussing sensitive or personal opinions shared in the group elsewhere. This information is also included in the revised manuscript on p.16, lines 400-409.

It is also not clear how long the participants were given before informed consent was obtained.

The health promoter contacted the potential families two weeks ahead of the first meeting. This information is now included in the revised manuscript on p. 9, line 240. Those families that accepted to participate attended the first meeting. All families invited by the health promoter accepted to participate aside of this they also invited other families they knew with children of similar age to the consecutive meetings. Informed consent were signed on the first occasion the families participated.

The data analysis is based on rapid identification of themes from audio-recordings (RITA) which is an appropriate method which also includes method and investigator triangulation and is in accordance with the Lincoln and Guba's (1989) framework of quality criteria of credibility, transferability, dependability and confirmability.
The limitations relating to selection bias and the potential for a conflict of interest by including a private oral health company are acknowledged and have been sufficiently justified by the authors.

Thank you for the comments.

The transcript could be improved by proof-reading as there are some instances when the definite article is missing e.g. page 12 (line 10) and typographical errors e.g. page 15 (line 35). Some statements made in the introduction are not supported by citations e.g. page 5 (line 50). Data could also be included to support statements.

Thank you for bringing up this. We have looked through the typological errors and also included references to statements were necessary.

Response to the second reviewer

1) Overall, the topic is interesting and the research team has uncovered unexpected findings that reinforce the importance of this kind of qualitative investigation/intervention. The approach is novel and fresh, a welcome change from more traditional approaches.

The authors are grateful to the reviewers for their encouraging words and interesting comments.

2) The introduction is well written, but as bit long. It is good and it does set the stage. Could these main points be covered with fewer words?

We have done our best in carefully reducing the number of words in the introduction.

3) The qualitative methods used is not my area of expertise, so another reviewer should comment on this area. It would be nice to be more transparent and see a table or graphic to show the percent of time certain themes were expressed, or when they emerged over the course of the study.

Thank you for the suggestion. However, the focus group discussions were analyzed using quality content analysis with an inductive approach following the guidelines of David R. Thomas. This type of analysis does not necessitate quantifying codes that fall under the different themes. Additionally we did not use computer programs like N-Vivo or similar which support quantification of codes from transcripts since we did not transcribe our material in accordance to the data extraction method we used namely rapid identification of themes from audio recordings.

4) The quotes in the results section are excellent.
Thank you for the comment!

5) Also, a diagram that describes the time frame of the study, the number of participants and number of meetings would be helpful.
Thank you for the suggestion. A time line showing the various events has been included as a supplementary file to the manuscript.

6) The word count, 7370, far exceeds the suggested 4000 word max. Could some details be included in a supplemental section?

We would like to bring to your notice that this manuscript is based on a CBPR approach that necessitates detailed description of the partnership process, which precedes the development of health promotional activities, unlike traditional behavioural interventions. Furthermore, the current study also presents results from nine large focus group interviews making the content both rich as well as extensive. Therefore, we argue that we like to keep this somewhat longer manuscript as we are afraid it may affect the quality of the manuscript should it be shortened to fit the 4000 word limit. However, taken into the account the valuable comments provided we have done our utmost to revise repetitive text and shortened the manuscript wherever it has been possible.

7) In the Abstract, the first sentence of the Results seems to lack face-value clarity,

The sentence is rewritten as follows in the abstract section of the revised manuscript. Three main themes emerged from the analysis which provided an understanding of the determinants for behavioral change including meaningful social interactions, family dynamics, and health trajectories.

and the second sentence appears to imply that one main theme may not have not been well

defined--which suggests ambivalence by the authors, or a lack of clarity.

Thank you for this comment. We have rephrased this sentence as follows to make the message clearer in the revised manuscript. The mothers in the study appreciated the social aspects associated with their participation; however, they also believed that gaining knowledge together with social interaction, made their presence also meaningful.

For example,
"family dynamics," while brief, seems imprecise—could one or two words be added to ground these concepts for greater clarity to the reader (without needing to read the full text)? For example, "Passive stances and dysfunctional family assumptions/dynamics"—(or a phrase describing the effects of patriarchy)—perhaps this implies a negative judgement, but hopefully you get the idea.

Thank you for the suggestion. However, we prefer not to turn the focus on more traditional aspects such as patriarchy. We believe that family dynamics still explains the results since it is about the role of individual family members and the relationship between members in the family and how this contributes to certain health behaviors in children.

8) The discussion has some areas that need to be addressed.
a. There are many areas that read, for example, "…this led to increased empowerment" (not an exact quote). As empowerment was not measured quantitatively, such comments can not be made with certainty. Yes, I agree, it appears that empowerment was increased, but, in this context, behavior change is a "black box"--perhaps assertiveness and questioning patriarchal stances led to the change. It would be fair to say that, "participants reported and increase in X, Y or Z", or that "there appeared to be an increase in empowerment". But at present, some conclusions are stated with too much certainty given what was done, measured and observed.

Thank you for this suggestion. We have read through the discussion section and have made sure that we are more cautious and that we discuss results in a passive tone.

Another example (p 22), "The interaction between
individuals in the group [appeared to] exert a strong influence on behaviors…"

The suggested change has been made on p. 24, line 584.

In the Conclusion (p 26), the phrase "…empowers individuals…" could read
"..appeared to empower individuals…"
We have rewritten the sentence as suggested on p. 28, line 680.

b. Consider making a case for empowerment in the conclusion and giving it its own paragraph pulling off of data.

We have included a few sentences on empowerment in the conclusion section of the revised manuscript on p. 28, lines 682-685.

c. The writing in the Discussion is wordy and indirect; there are many run-on sentences before and after citations #62, #63 and at the top of page 23 ("The stages leading to…") As a reader, I lost interest. This needs to be re-written and made more concise, precise and pithy. Please remove the redundancy throughout

the text especially in the Discussion.

The authors have attempted to critically read the discussion session and revise when considered appropriate. We would like to thank the reviewers for this feedback since the discussion section reads better than before.

d. Mention TePe and why this person was important and did not bias the study in the methods, not at the end of the text—it call more attention to itself there.

We have moved the text to the methods section on p. 10, lines 258-271.
e. The lack of quantitative assessment is a major limitation and should be listed first; order limitations in terms of their relative or overall importance.

The limitations section has been rearranged as suggested.

9) Please document how long each meeting was. This is important when comparing these data to traditional and less time-intensive dental visits.

This information is included in the method section on p. 10, lines 252-253.

10) I suggest having people not associated with the study proof-read the manuscript to identify areas that are vague, unclear or repetitive.

Thank you for the recommendation. We have requested some of our colleagues to read and comment on our manuscript. This has also helped with improving the manuscript.

VERSION 2 – REVIEW

REVIEWER	Anne Marie Coll School of Care Science Faculty of Life Science and Education University of South Wales Glyntaff Pontypridd Mid Glamorga
REVIEW RETURNED	20-Feb-2020

GENERAL COMMENTS	This re-submission is much improved. There are however a number of minor typographical and spelling errors I would like to bring to the authors' attention. p3 line 101 practising not practicing p6 line 178 were not was p8 line 211 disadvantaged not disadvantage p10 line 262 remove 'of' p11 line 276 add 'a' before power-balanced... p12 line 310 add apostrophe after participants p13 line 327 add comma after hygiene p13 line 334 remove 'e' p14 line 370 add 'as' after such p15 line 385 were addressed not was address p17 line 441 add a full stop p19 line 480 add 'a' after as p19 line 486 add 'the' before parent's p21 line 530 remove apostrophe before sugar

	p22 line 547 add 'a' after reported p23 line 577 where not were p23 line 578 were not was p23 line 579 where not were p24 line 595 findings not finding p24 line 601 add 'the' after enhances p25 line 607 add 'a' before healthier p25 line 615 remove 'and' p27 line 655 add 'the' before significant p27 line 662 add 'a' after also p27 line 669 add apostrophe after participants

VERSION 2 – AUTHOR RESPONSE

This re-submission is much improved. There are however a number of minor typographical and spelling errors I would like to bring to the authors' attention.

The authors would like to thank the reviewer for her feedback and suggestions.

p3 line 101 practising not practicing

p6 line 178 were not was

p8 line 211 disadvantaged not disadvantage

p11 line 276 add 'a' before power-balanced...

p12 line 310 add apostrophe after participants

p13 line 327 add comma after hygiene

p14 line 370 add 'as' after such

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p23 line 579 where not were

p24 line 595 findings not finding

p24 line 601 add 'the' after enhances

p25 line 607 add 'a' before healthier

p27 line 655 add 'the' before significant

p27 line 662 add 'a' after also

p27 line 669 add apostrophe after participants

The changes enlisted above have been made in the revised manuscript as suggested.

p10 line 262 remove 'of'

p13 line 334 remove 'e'

p21 line 530 remove apostrophe before sugar

p25 line 615 remove 'and'

The text have been removed as recommended