Author’s response to reviews

Title: Addressing fragility through community-based health programmes: insights from two qualitative case study evaluations in South Sudan and Haiti

Authors:

Séverine Erismann (severine.erismann@swisstph.ch)
Sibel Gürler (sibel.guerler@swisspeace.ch)
Verena Wieland (verena.wieland@redcross.ch)
Helen Prytherch (helen.prytherch@swisstph.ch)
Nino Künzli (nino.kuenzli@swisstph.ch)
Juerg Utzinger (juerg.utzinger@swisstph.ch)
Bernadette Peterhans (bernadette.peterhans@swisstph.ch)

Version: 1 Date: 12 Jan 2019

Author’s response to reviews:

We have uploaded a point by point response for the reviewers. Reference Number HRPS-D-18-00142

Erismann et al. “Addressing fragility through community-based health programmes: insights from two qualitative case study evaluations in South Sudan and Haiti” (your reference no. HRPS-D-18-00142) – Point-by-point response

Basel, 12 January 2019

Dear Dr. Gonzalez-Mcquire
We were delighted to receive your e-mail dated 18 October 2018 in connection with the aforementioned manuscript. We thank you and the three external reviewers very much indeed for carefully studying our piece and providing a series of most useful comments and suggestions. We are grateful for the invitation to re-submit a revised version of our manuscript.

In the meantime, we have substantially reworked our piece in light of the Chief Editors and the three external reviewers’ reports. Below, please find our point-by-point response, clearly indicating how and where in the manuscript changes have been made. Due to the major changes also in the structure of our piece, the track changes are numerous. We also enclose a “clean” version of the revised manuscript as supporting information. Please note that the line numbers where changes occurred refer to the clean version of our piece.

We look forward to your further disposition and would be thrilled if our revised manuscript would be accepted for publication in the open-access journal Health Research Policy and Systems.

Yours sincerely,

Séverine Erismann and Bernadette Peterhans
(on behalf of all authors)

Dear Mrs. Peterhans,

Your manuscript "Community-based health programmes in fragile contexts: insights from two qualitative case study evaluations in South Sudan and Haiti" (HRPS-D-18-00142) has been assessed by our reviewers. Although it is of interest, we are unable to consider it for publication in its current form. The reviewers have raised a number of points which we believe would improve the manuscript and may allow a revised version to be published in Health Research Policy and Systems.

We were delighted to learn that our manuscript is deemed of interest for the readership of Health Research Policy and Systems, and hence, we are grateful for the invitation to revise and resubmit our piece.
Their reports, together with any other comments, are below. Please also take a moment to check our website at:

https://scanmail.trustwave.com/?c=6967&d=y4bI250XdeO6OcnHo2za6xNjIT8Ydkud7uO3Wu8YHA&u=https%3a%2f%2fhrps%2eeditorialmanager%2ecom%2f

for any additional comments that were saved as attachments.

If you are able to fully address these points, we would encourage you to submit a revised manuscript to Health Research Policy and Systems. Once you have made the necessary corrections, please submit online at:

https://scanmail.trustwave.com/?c=6967&d=y4bI250XdeO6OcnHo2za6xNjIT8Ydkud7uO3Wu8YHA&u=https%3a%2f%2fhrps%2eeditorialmanager%2ecom%2f

Please include a point-by-point response within the 'Response to Reviewers' box in the submission system and highlight (with 'tracked changes/coloured/underlines/highlighted text) all changes made when revising the manuscript. Please ensure you describe additional experiments that were carried out and include a detailed rebuttal of any criticisms or requested revisions that you disagreed with. Please also ensure that your revised manuscript conforms to the journal style, which can be found in the Submission Guidelines on the journal homepage.

Thank you very much for providing detailed reports from three external reviewers, which formed the basis for our comprehensive revisions. The point-by-point response is given below. As per your instructions, we have resubmitted a track-changed version. Additionally, we uploaded a final clean version. Please note that the line numbers specified in our point-by-point response correspond to the final clean version.

The due date for submitting the revised version of your article is 17 Nov 2018. This deadline may be extended upon specific request.

We thank the Editors for having granted us an extension of the deadline for resubmission until 31 December 2018. Due to some technical issues while attempting to upload our revisions, we needed a few extra days (final resubmission on 12 January 2019).

Please note, if your manuscript is accepted you will not be able to make any changes to the authors, or order of authors, of your manuscript once the editor has accepted your manuscript for publication. If you wish to make any changes to authorship before you resubmit your revisions, please reply to this email and ask for a 'Request for change in authorship' form which should be completed by all authors (including those to be removed) and returned to this email address.
Please ensure that any changes in authorship fulfil the criteria for authorship as outlined in BioMed Central's editorial policies.

Once you have completed and returned the form, your request will be considered and you will be advised whether the requested changes will be allowed.

By resubmitting your manuscript you confirm that all author details on the revised version are correct, that all authors have agreed to authorship and order of authorship for this manuscript and that all authors have the appropriate permissions and rights to the reported data.

No changes have been made regarding the authors and order of authorship. Hence, no action has been taken in this regard.

Please be aware that we may investigate, or ask your institute to investigate, any unauthorised attempts to change authorship or discrepancies in authorship between the submitted and revised versions of your manuscript.

We agree and confirm that author details on our revised version are accurate.

I look forward to receiving your revised manuscript soon.

Best wishes,

Rosanna Gonzalez-Mcquire

Health Research Policy and Systems

Many thanks again, dear Dr. Gonzalez-Mcquire, for the constructive feedback and for handling our manuscript in a timely manner— we highly appreciate it!

Reviewer reports:

From the Chief Editors: Additional emphasis on the policy aspects would be beneficial for this article.
We have addressed this comment and put additional emphasis on the policy aspects in the discussion and recommendation sections of our revised manuscript: “Fifth, advocacy for the establishment and strengthening of adequate coordination mechanism has to be increased to avoid the creation of inequalities on the ground, which can lead to tensions and further deterioration of social cohesion. Moreover, exchange across the various aid actors (relief, development and peace and statebuilding actors) is vital for overall more coherent approaches and policies where aid organisations interfering in a context become mutually reinforcing instead of mutually weakening” (see revised manuscript, lines 640-645).

Reviewer 1:

1. This article addresses an interesting concept - can access to predictable health services actually make a country less fragile. Yet it does so in an unstructured way. Definitions of fragility and the fragility framework are only briefly described and not in enough detail for the reader to understand the framework.

Point is well taken and definitions have been added in Table 2.

2. The study is based upon qualitative data of 49 participants, which is fine, but then strong statements are made regarding the findings, such as "Results...presented here suggest that community-based health programmes in fragile contexts can contribute to mitigating fragility." (page 15, 391-392).

We addressed this comment and reformulated: “Qualitative data from the two case studies presented here suggest that community-based health programmes in fragile contexts may have influenced certain drivers of fragility” (see revised manuscript, lines 476-477, as well as Abstract, lines 34-35).

3. There is no mention that it could be the other way around - less fragile contexts are more amenable to community-based health programmes.

We have introduced the possibility that less fragile contexts are more amenable to community-based health programmes in the introduction of our revised manuscript (working around fragility) (see revised manuscript, lines 71-77). As both Haiti and South Sudan were at times of this study highly fragile and as we did not assess or measure the extent of fragility in these contexts, this point was not further elaborated in the Discussion.
4. The article begins (page 3, lines 54-55) with a significant misunderstanding of the facts, stating that "the proportion of people living in fragile contexts is expected to increase from 17% at present to 46% in 2030, mostly affecting the extreme poor". This is not what the World Bank report states. Rather, the report states that "By 2030, the share of global poor living in fragile and conflict-affected situations is projected to reach 46%." This is a major error in facts. Global poverty rates are declining. The UN through the Sustainable Development Goals estimates that 11% of the world population live below the extreme poverty threshold. The World Bank report states that by 2030 about half of the extremely poor will live in fragile contexts, not that 46% of the world's population will live in fragile contexts.

We fully agree with Reviewer #1. While revising and restructuring the Background section, we have omitted these sentences (see revised manuscript, lines 52-62).

5. The fragility framework needs to be presented in greater detail. The drivers are listed on page 9 lines 231-236 but the reader needs more understanding. How do they drive fragility? How has this framework been tested or used? What evidence is there to support the framework? Can examples be given of countries that fit the various criteria?

We addressed this issue and now provide more details to the key drivers of fragility and examples of how these apply to the two case study countries South Sudan and Haiti. The fragility framework was elaborated with the results of the interviews conducted in each country. Hence, we have reworked the corresponding sections and now present more details to the key drivers of fragility identified and provide specific examples of how these apply to the two case study countries. A table outlining how the different drivers contribute to fragility, including the underlying evidence based on the extant literature is given (see revised manuscript, lines 317-470 and new Table 5).

6. Page 9, line 243-245 is an erroneous statement. The OECD report states that "more countries experienced some form of violent conflict than at any time in the past 30 years". However, there is not a general historical trend in increasing violence.

We have reworked this paragraph and omitted this sentence. As highlighted in our response to point#1, definitions of fragility have been added in the new Table 2 (see revised manuscript, Table 2).

7. Without a stronger foundation of the fragility framework, the various elements in the Results section (i.e., effective mechanisms to ensure inclusive participation, etc.) give little evidence of
whether the experiences of Haiti and South Sudan actually limit fragility. These elements need to be presented within the framework.

As detailed in our response to point #5, we have presented the key drivers of fragility in greater depth. Additionally, we restructured the Discussion and now highlight respective results in context-specific settings (see revised manuscript, lines 317-470 and lines 486-571 and Table 5).

8. The evidence is not strong enough to come to the conclusions of the Discussion section, for example, page 15 lines 409-410. The entire Discussion and Conclusion sections require more careful and neutral analyses, especially regarding confounding factors.

We addressed this point by carefully revising and toning down the discussion and the conclusion parts (see revised manuscript, Discussion and Conclusions sections).

Reviewer: 2

9. Authors aim to address the issue if health programmes in fragile environments can contribute, beyond health gains, to mitigating fragility. This link is often assumed and feels intuitively correct, but is notoriously difficult to prove. It is therefore highly laudable that authors have tried to address this issue by looking at two case studies, in South Sudan and Haiti. And much of the discussion, with its set of recommendations, and conclusion read as sound advise, good practice. However, this advice is not really grounded in the study (the two case studies) itself. The two case studies are not very conclusive, with the 'more' conclusive one, South Sudan, being hampered by serious flare up of hostility in the study area, resulting in inability to visit the study area, and limits of involvement of local representatives among the interviewees.

We thank Reviewer #2 for this comment. We agree that the outcomes of the two case studies are context-specific and face several limitations, which we clearly present in the introduction and the methodology of the article (study design and sampling) and in the Discussion/limitations section. In light of the Chief Editors and the three external reviewers’ comments, we substantially reworked and restructured the Discussion section in order to avoid generalizations and to provide stronger links to the key outcomes of our research (see revised manuscript, Results and Discussion sections).

10. Recommendations and conclusion seem to be more grounded in a model (drivers of fragility) that authors (in particular swisspeace(?)) have developed and review of literature. If at all this would be acceptable to the journal, I would therefore tend to advice to rework the manuscript. Start with a much more explicit declaration and explanation of the model, followed by 'modelled'
consequences for (health) programming - as have now largely found a place in recommendations and conclusions. The case studies could then be presented as a reality check of the model and its programming consequences. Discussion and conclusion could then focus on both adaptations of the model as well as recommendation for programming for similar (small scale) projects as described in the two case studies.

This is a very interesting recommendation. We considered the suggestion of starting the article with the description a general model. Yet, it does not fit with the methodological approach of the two case study evaluations for two main reasons. First, the context and drivers of fragility for both countries had to be identified during the study. Second, these contexts are widely different and a general model would not be suitable to both. However, we have further clarified the methodological approach of the two case studies, as already detailed in our response to point #5. Please note that the drivers of fragility were identified after having conducted the interviews. Hence, we have reworked the Discussion and Recommendation sections (see revised manuscript, lines 473-611 and lines 614-645).

Reviewer: 3

11. The topic of fragile contexts and the potential of adding transformative objectives such as state building to health interventions is taking an important place in policy discussions. However, there is much rhetoric and wishful projection in the current policy debate, reflecting donor and other political agenda's. The very narrow evidence base therefor needs strengthening, by bringing evidence and field reality to balance and compensate for opinions, discourse and projections of possible benefits.

Unfortunately the submitted article does not bring rigorous evidence and due to important bias and gaps in methodology, instead risks to induct more confusion.

The first problem is that two very different contexts are grouped together. The wide range of contexts within the OECD category of FCAS doesn't really allow for generalisations. The differences between Haiti and South Sudan in terms of underlying crisis (natural disaster versus conflict), pre-existing health and community systems, time elapsed since acute stage of crisis, etc make it difficult to lump together any outcomes and draw overarching conclusions for FCAS.

We thank Reviewer #3 for this comment. As suggested, we have now more consistently separated the Haiti and South Sudan case study findings to avoid making overall generalizations, conclusions or recommendations and introduced the discussion section as follows: “The findings of the case studies and implications are discussed, taking into consideration idiosyncrasies between South Sudan and Haiti regarding the underlying crisis” (see revised manuscript, lines 484-485).
12. The greatest limitation and introduction of potential bias - nowhere explicitly mentioned - resides in the fact that the overwhelming majority of people interviewed are part of the organisation or direct partners. Both implementers and donors are likely to hold a positive bias towards interventions they conduct and want to continue to receive support for. In that sense, the qualitative research is mainly giving information on how they perceive the interventions and not on what really these interventions brought as change. It would have been interesting to bring out some dissident voices instead of showing mainly the common perceptions.

This point has been addressed by clearly indicating that the people interviewed might have introduced some bias, which is an important limitation of our study. We added the following text: “The two case studies included 49 participants (13 females and 36 males; 19 from South Sudan and 30 from Haiti). Interviewees’ affiliations are summarised in Table 4. Of note, most of the participants were part of the SSRC, the SRC or a partner organization, while only few beneficiaries were interviewed (exclusively in Haiti) (see revised manuscript, lines 253-255). We have also revised the Discussion/limitations section, as follows: “Second, the violent outbreaks in South Sudan shortly before the fact-finding mission may have influenced responses and limited data collection in Mayendit county. As in the case of South Sudan, only primarily programme implementers, donors or employees from SRC were interviewed, and hence, the analysis does not include any beneficiary perception, and consequently, there may be a bias of the study participants towards the interventions they conducted” (see revised manuscript, lines 649-653).

13. Throughout the study there is no clear indication what measures are used to gauge if contribution to state building occurred. There is no real clarity to which findings from interviews and focus group discussions will be counted as such. It would have been useful eg to bring forward what expected results were checked for or even proposed in the project documents.

It is an important question how one could measure in an objective way if results are obtained - but the article doesn't create any clarity on what could be useful indicators to monitor. Maybe as a consequence, the language used in the article is cautious (eg suggest, were able to, seems important) which is correct, but still suggests findings without real measured basis.

The objectives of the case study evaluations were to identify key drivers of fragility and to assess the interaction of the health programmes within the specific contexts of South Sudan and Haiti. The methodological approach of this programme evaluation was not designed to measure or monitor indicators of state-building, but to identify key drivers of fragility and assess the influence or potential effects of the health programmes on these drivers. We believe that these objectives are clearly stated, and hence, no further action has been taken.
14. Regularly the article states 'besides reaching health objectives' but there are no indications given on actual impact on equity and/or service utilisation. Even if the article focuses on the effects beyond the health effects, this is an important question, as one of the risks of combined health and other objectives beyond health would be that such state building objectives might push out most effective health interventions, amongst others by choosing approaches favouring transformative goals but less effective for health progress.

The objectives of the Swiss Red Cross projects were focused on improving access to health services in places where these were not available (i.e. in areas destroyed by the earthquake in Haiti and in areas without any primary health care unit in South Sudan). In this context, improving equity and/or service utilisation were at the core of the SRC project interventions. However, Reviewer #3 is correct in pointing out that we did not assess the effects of the SRC projects on an impact level, an endeavor that in these contexts would have been methodologically very challenging (i.e. emergency situation in Haiti and no primary health care unit in South Sudan). Nevertheless, we believe it is important to move from sectoral interventions to improving health to considering objectives beyond health, especially in fragile context where health inequities require considering its context-specific causes. As stated in our Results and Discussion sections, the qualitative study design was chosen to assess the interaction between the health programmes and the fragility context. We have now further clarified this section as follows: “While impacts cannot be measured or quantified in the absence of a baseline (the projects were not originally designed to mitigate overall fragility), the study, nevertheless, reveals entry points for designing programmes that are responsive to the overall fragility context and contain more specific elements for navigating a more sustainable pathway out of fragility” (see revised manuscript, lines 36-39 and lines 478-481).

15. The situation in South Sudan, with the government part of the warring parties in the conflict, would be interesting to explore in terms of the statebuilding objectives itself; would this be a disqualifying factor and/or what limitations in close collaborations with State representatives would this imply? Would this also limit the possibilities to intervene in certain geographic areas or ethnic groups? How to interpret the acute conflict resumption and what consequences this has for the project in the previous set-up and ambitions? Did the state building approach cause any difficulties in returning to humanitarian aid (e.g. in perception of impartiality)? Could any mitigation be observed in violence in the communities where the project was located?

These research questions, even though highly interesting, were clearly out of the scope of this programme evaluation of the two case studies. However the Swiss Red Cross does work with all ethnic groups and tries to balance the interventions to mitigate fragility and not to contribute to possible conflicts among different groups. Within the Red Cross Movement the International Committee of the Red Cross (ICRC) works mainly in conflict areas (humanitarian aid), whereas
the Federation and the National Societies work towards strengthen the local Red Cross Society and therefore focus on linking relief, rehabilitation and development with a focus on the most vulnerable and marginalised population groups. Changing the focus rapidly within the Red Cross Movement from development to humanitarian aid did not cause major difficulties. However, disruption of programmes are sometimes unavoidable. On the other hand, the local counterparts normally get quickly in contact with the Red Cross when the situation changes. For example, in South Sudan, ICRC handed over the primary health care programmes to the national societies as soon as the peace agreement was signed and, in 2013, ICRC did take over from the National Societies when the area was not accessible the conflict broke out.

16. Negative findings or fundamental questions raised by the interviewees/focus group participants get very little attention and are not reflected in discussion. One area seems the 'non-understanding' of the importance and interpretation given to 'sustainability' by the project. From some interviews, it seems the approach might have created more confusion in the community on what the project contributed in benefits for the community; possibly this lack of clarity might also induce loss of accountability.

We agree that the non-understanding of the importance and interpretation given to the issue of sustainability by the project has not been taken up in the Recommendations. While revising our piece, we added the following sentences: “Third, the issue of the sustainability of interventions has been raised at the beginning of project design stages with project partners, stakeholders and beneficiaries. Engaging in community dialogues about the intervention may not only enhance community’s capacity to integrate interventions into existing practices, but also to address underlying elements, such as socio-cultural/community context, or the organisational settings that can support the sustainability of interventions [71]. Programmes should fit with existing community resources and should involve state authorities from the local up to the national levels to safeguard proper hand-over strategies that, moreover, promote state-led development” (see revised manuscript, lines 628-635).

17. Within the discussion part, the recommendations on flexibility etc make perfect sense but have little linkage with the reported outcomes of research.

We agree with Reviewer #3, and hence, we have omitted this recommendation. As stated in our response to point #9 of Reviewer #2, the reported outcomes of our research are now better linked to the recommendations (see revised manuscript, lines 317-470, lines 473-611, lines 613-645).
While carefully revising our piece – word-by-word, sentence-by-sentence, additional changes have been made and this are highlighted using the Track Changes mode. Moreover, several additional references have been cited, as indicated below.

References


