Author's response to reviews

Title: The Mental Health, Quality of Life and Life Satisfaction of Internally Displaced Persons living in Nakuru County, Kenya

Authors:

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Author's response to reviews: see over
Reviewer 1

Reviewer comment: Abstract: Line 23 - 'very poor' is a subjective term, consider revising (e.g. ...can cause problems with mental health and wellbeing)

Authors’ response: Thank you. This has now been amended.

Reviewer comment: Abstract: Line 23 - I don't think you can draw a clear connection to conflict-induced forced displacement in this article, please see further comments on this in the background section of the main text.

Authors’ response: We respectfully disagree with this comment. The IDPs in this study were forced into internal displacement due to an ethnic conflict that resulted from a political crisis.

Reviewer comment: Line 28 - See comments on the cross-sectional questionnaire in the methods section of the main text, will require revision of this. The abstract will require revision once the comments on the main text is addressed.

Authors’ response: Thank you. The abstract has been amended accordingly.

Reviewer comment: …the Kenyan crisis can be firmly categorised as political/civil unrest. There may have been elements of ethnic conflict, but I don't see how it can be classified as a full-scale conflict or war (in its purest sense)….I think you will need to revise the background (especially the second paragraph) and discuss the impact of civil unrest/violence that causes internal displacement on mental health, rather than allude to the broader war related impact. While your study stands to add to the evidence specific to this area, I would avoid sweeping statements about the broader contribution.

Authors’ response: We agree that framing our study as providing evidence to the war-based conflict (in its purest sense) research base is inappropriate. We have therefore reframed the introduction to reflect this shift in focus which now strictly centres on ethnic violence and conflict as a result of civil and political unrest (rather than war).

Reviewer comment: Methods: Please remove all information about the questionnaire from this section and include them under a subheading titled 'Measurements or Instruments'. Design should only describe the study design, not the questionnaire.

Authors’ response: Thank you. This has now been amended

Reviewer comment: I have concerns about calling this a cross-sectional survey, although the design broadly is cross-sectional, the usage of open-ended questions and using qualitative analyses would probably require you to describe this as a mixed-methods study in my opinion. If purely cross-sectional, the sampling approach creates big questions.

Authors’ response: Thank you and we agree. This has now been amended to reflect the mixed-methods nature of the study approach.

Reviewer comment: Ethics information should come after data analysis.

Authors’ response: Thank you. This has now been amended.
Reviewer comment: Are the instruments used (GHQ, WHOQOL-BREF and SWLS) validated for Kenyan population? Have they been used in Kenya before?

Authors’ response: The GHQ-12 has been previously validated for the Kenyan population. The WHOQOL-BREF has been previously used in Kenya but not formally tested for validity or reliability in this population. The SWLS has not been previously used or tested for validity or reliability in this population. These points have now been stated within the instruments sub-section.

Reviewer comment: I am assuming that it was given to participants in English? Does everyone in Kenya speak and understand English? Was all your IDP population able to understand the questions and answers? How was it administered - was it self-report or was it an interview? Did you only recruit people with English Knowledge? If so, what are the implications of this on your findings?

Authors’ response: Yes, the questionnaire was prepared and delivered in English – this has now been made more explicit within the new ‘instruments’ subsection. As stated in this section, English is one of the official languages in Kenya and as such the large majority of the population can speak and understand English, hence why it was decided that using this language alone was sufficient. However the researcher who was recruiting participants is fluent in Swahili and the two local dialects Kikisii and Kikuyu. Therefore he was on-hand to verbally translate the questionnaire if it was required. While no such request for translation was made we cannot completely rule out that some of those who refused to participate could not understand English. This point – and the implication this has for possible sample bias – has now been added to the limitations section.

Reviewer comment: Line 128-132 - Irrelevant information to the study being described.

Authors’ response: Thank you. This information has now been deleted.

Reviewer comment: This section would benefit from some information about mental health condition in the area, if anything is available, or some comparative information from Kenya. Also, maybe a bit more information about provision of mental health care services in the area would be helpful. Most importantly, numbers should be provided about the IDP situation (there is some mention about 30,000 IDP in Nakuru in the background section - perhaps that can be moved here) and especially from line 140 onwards, numbers needed, as to how many IDPs, how many camps, evidence of overcrowding etc. Basically, a more compelling case needs to be presented about the setting.

Authors’ response: We have added information about the mental health context in Kenya and also highlighted the challenge in IDPs accessing mental health services in Nakuru. We have moved and amended the information concerning numbers of IDPs in Nakuru County to the setting section as advised. Information about the number of camps has been added, and a clearer description of the nature of these camps has been provided with citation. Within the sampling section we also described the total number of tents among the four camps sampled in this study.

Reviewer comment: Sampling: Either here or in limitations, you will need to explain your reasons behind choosing purposive sampling, why there wasnt any sampling calculation etc.
Authors’ response: We have now explained this in the limitations section.

Reviewer comment: Please explain the justification of camp selection, were they selected based on their population size, easy access, some other reason?

Authors’ response: Due to limited resources we were unable to sample more than four camps. Therefore we purposefully selected the Nawam, Tuinuane, Jikaze, and Minto camps because of their geographical spread across the county that we believed would better represent the county as a whole, compared to four camps in situated in one region of the county (this is stated in the sampling section). However, we do acknowledge that despite this strategy that our sample lacks representativeness, as stated in the limitations section.

Reviewer comment: Also, please justify the tent selection, this appears to be completely ad-hoc. There are many ways of random household (or tent) selection in surveys, was there any reasoning behind this?

Authors’ response: While we were aware that we could not sample any more than four camps for practical reasons, we wanted to build in random sampling at the household/tent – level to strengthen representativeness. Systematic sampling was selected as it is a simple and widely used means towards enabling all tents having an equal chance of being selected. The interval of 3 was purposefully selected to enable us to approach as many tents as practically possible within the limited time available to us during our recruitment and data collection period.

Reviewer comment: The other issue is the coin-toss to select females or males, isn't the idea to capture the actual representation of the population? Your justification should be provided, and the reasoning behind trying to recruit equal female:male numbers.

Authors’ response: The coin-toss strategy was built in to increase the chances of equal participation between males and females, and also because latest census data in this county show an even male-female ratio. This is now clarified in the setting and sampling subsections.

Reviewer comment: Also, in the results and later, you mention that your male representation was lower, did the sampling strategy have a role in this? or does your numbers represent the actual situation in the camps or in the overall Kenyan population?

Authors’ response: The key reason male participation ended up being lower was because many of the males were not available during the day. The problem was not the coin-toss strategy itself but rather the fact that we were unable to recruit people other than during daytime hours because of evening security curfews in each camp. We have amended the limitations section to make this point as clear as possible.

Reviewer comment: Data analyses: Line 161-162 - This relates back to sampling, did you consider this possibility of data not representative of the target population when you came up with your sampling strategy? That probably is the reason why your data was not representative, and you clearly should have envisaged this issue during the design. You will need to address this in detail in your limitations sections.
Authors’ response: Yes, we knew from the outset that we had limited time and resources to construct a representative sample hence why we purposefully selected the four camps (as described in one of our responses above). The issue of limited time and resources as the key reason for why four camps were purposefully selected has now been added to the limitations section.

Reviewer comment: In the qualitative analysis, did at least two researchers code themes independent of each other?

Authors’ response: No, only CP conducted the thematic analysis as he has expertise in this analytical method. The use of two independent coders is a debated one: http://www.psych.auckland.ac.nz/en/about/our-research/research-groups/thematic-analysis/frequently-asked-questions-8.html#c83c77d6d1c625135085e489bd66e765

Reviewer comment: Ethics: Was ethical approval from a local Kenyan ethics committee obtained? If not, this raises questions about the ethical conduct. It is now globally accepted that in international collaborative research, ethical approval should be obtained at both ends; from an ethics committee in a western country whose researchers have a leading collaborative role, and also from a committee in the developing country where the research is conducted. Please see Nuffield Council of Bioethics guidelines on this. If Kenya didn’t have a single ethics committee (which I believe is not the case), then having only UK approval can be deemed sufficient. Informing local leaders and administrative bodies is not sufficient ethical oversight, especially in mental health research among such vulnerable groups as IDPs, as there as extremely high risk of exploitation.

Authors’ response: Ethical approval was not sought from a Kenyan ethics committee which we now recognise to be an oversight which we thank you for highlighting. We also recognise that this may raise questions about ethical conduct. However the Committee from which ethics approval was sought and obtained from is highly rigorous and required from us our research protocol, data collection instruments, and evidence that camp leaders and administrative officials were happy to support the study and provide safe access to camps. These officials also requested that they review our protocol and data collection instruments, as well as interview the lead researcher on site (EM). All prospective participants were treated highly sensitively, were provided participant information forms that clearly explained the purpose of the study, what is involved, how data will be treated, and what will happen after the data had been completed. All participants who agreed to participate signed informed consent documents and understood that their participation was voluntary, anonymous, that data was be treated with confidentiality, and that they were free to withdraw from the study at any time and for no reason. These processes went very smoothly and to the best of our knowledge no complaints were made about our conduct to camp officials. We are sorry that we did not also seek approval from a Kenyan ethics committee and recognise the implications. However our study was conducted ethically and we believe that approval from University of Bedfordshire’s ethics committee and the camp leaders reflects this. We have now also added to the ethics approval section some further clarification on our ethical conduct.

Reviewer comment: Results: Line 172-179 - The numbers given here about number of participants from the four camps do not agree with what is given in table 1. For example, in table 1, it says 100 were approached, and 68 participated from Nawam camp while 81 were
approached in Tuinuane camp and 58 participated. However, results section says a total of 100 IDP were recruited overall with 36 from Nawam and 35 from Tuinuane?

Authors’ response: We sincerely apologise about this error and thank you for bringing it to our attention. The correct figures are now described in table 1.

Reviewer comment: Also, results section says a total of 179 tents were approached altogether, but if you look at the table, the total of tents approached in the two camps mentioned alone is 181?

Authors’ response: The correct figures are now described in table 1.

Reviewer comment: Also, in the abstract you give a response rate of 72% while in the results (line 178) there is a 69% response rate of tents. Also, since you dont have a sample calculation and an estimated sample, how did you calculate a response rate? These are only a couple of serious issues in the results, about numbers in tables not corresponding to what's written in the text.

Authors’ response: Apologies – changes have now been made that now accurately describe these figures. Firstly we have deleted the abstract response rate statement. Secondly, we have made amendments that clarify that out of the 179 tents we approached, 100 agreed to participate (therefore a response rate of 55.9% as now stated on line 245 – this rate includes both refusers and non-contacts). These figures also now accurately reflect table 1.

Reviewer comment: Line 183-186 - Doesn't add up o 100, way over 100 in fact (45+35+28+10), did individual participants report multiple support sources?

Authors’ response: Yes, this is because individuals reported multiple support sources.

Reviewer comment: Line 208-212 - Qualitative findings need much more detail, suggest using a separate subheading to separate them from the quantitative. Your quantitative findings stand to be explained much strongly through these qualitative findings, so you need to explain them and provide more detail about these results than just a cursory report of few themes.

Authors’ response: Thank you. The identified qualitative themes have now been explained in more detail. We have also moved the quote examples in table 4 to this section as you suggest below.

Reviewer comment: Discussion: Line 217, 218 - Please avoid the usage of words such as 'strikingly' and 'remarkable', try to use objective wording

Authors’ response: Thank you. This has now been amended

Reviewer comment: Discussion overall is well-written, connecting qualitative and quantitative findings. However, the problem mentioned at the beginning in the background section about civil unrest creates issues here. The evidence cited are from mainstream conflicts and as I mentioned, limits the comparison with your findings, regardless of the IDP situation. While the Kenyan crisis may have precipitated IDPs to experience extreme violence, it can't be compared with mental health of Kosovar Albanians for example due to
the inherent contextual differences. I suggest revisiting the discussion, especially also considering comments from methods and results. Would be best to keep the discussion limited to discussing key findings in relation to situations of internal displacement due to civil unrest, or inter-communal violence, rather than compare with full-blown conflict settings.

**Authors’ response:** We have added in two studies whose IDP samples are drawn from similar conflicts to the type of ethno-political conflict documented in this study (see lines 322-331). We have also removed reference to the Lopes Cardoza study in this section from this section (lines 336-342). We have also added a statement highlighting how each of these described studies are based on conflicts which are contextually different but that, overall, the evidence from these studies support the notion of IDPs being at increased risk of poor mental health and reduced quality of life.

**Reviewer comment:** Limitations of the study require much more in-depth and detailed discussion, and not a superficial listing of few issues. Please take the comments from methods and results sections when you discuss limitations.

**Authors' response:** The limitations section has been expanded to capture issues put forward in previous comments.

**Reviewer comment:** Tables: General comment - please make sure the tables are in journal style formatting.

**Authors’ response:** Tables amended to better reflect journal style formatting

**Reviewer comment:** Table 1 - Please start the table with rows of gender, marital status etc. and remove camp information (maybe another table or figure detailing sampling strategy and recruitment numbers would be better)

**Authors’ response:** Thank you. Table 1 now only consist of camp and tent numbers while Table 2 now only includes participant background characteristics as requested. Changes to other effected table numbers also have been made across the manuscript.

**Reviewer comment:** Table 3 - Cant understand values given n table 3

**Authors’ response:** The legend beneath the table should help to understand what each statistic is referring to e.g. MR = mean rank, U = Mann Whitney U test score etc. Test scores which were significant at P<.05 level are starred for the reader.

**Reviewer comment:** Table 4 - I think this table should be deleted and use the quotes in the expanded qualitative results section as suggested above.

**Authors’ response:** Thank you. This table has now been deleted and quotes integrated into the expanded qualitative results section as suggested.

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**Reviewer 2**
Reviewer comment: In the introduction, more information is needed about:
- other studies regarding the mental health, quality of life and life satisfaction of IDP's
- possible conceptualisations of the concepts mental health, quality of life and life satisfaction, and the way these concepts are conceptualised in this study
- a rationale why the authors have chosen for these particular influencing variables (background factors, duration of displacement,...), and other studies regarding the impact of these factors on iDP's/refugees' mental health
- what is meant by "background factors" and by "perceived level of support"?
- which population/age groups/gender does the study target?

Authors’ response: Thank you. We have now cited a number of other relevant studies that evidence the links between poor mental health and wellbeing outcomes with the IDP populations. We have also provided rationale for why we collected data on particular factors – specifically that past research on IDP population has highlighted a number of potentially significant factors in determining mental health and wellbeing outcomes, such as age, gender and marital status (background factors), employment, duration of displacement, and perceived level of support. We have also made what we mean by ‘perceived level of support’ clearer by adding ‘from individuals and organisations’ to this statement (see lines 111 and 112). We also discuss the nature and importance of perceived level of support in the discussion section which adds further clarity.

Reviewer comment: Methods: More information is needed on the modifications to the WHO-QOL-BREF questionnaire

Authors’ response: Thank you. Further information has been added.

Reviewer comment: Was there a cross-culturally validation process of the questionnaires? If yes, how? if no, do we know what we measure? do we know that these questions are relevant for the population under study?

Authors’ response: Thank you. Information about each tool’s validation on a broad level and also on the Kenyan level has now been added.

Reviewer comment: ethical issues: what happened when people expressed certain needs, in particular mental health needs? where any provisions made to refer them to mental health care services?

Authors’ response: Thank you. We have now added some clarification on this point (see lines 232-235).

Reviewer comment: was a researcher present to ask all questions, or were the questionnaires given and then afterwards collected?

Authors’ response: The questionnaire were self-administrated so the researcher collected the completed questionnaire from participants later. If invited participants were unable to read and write in English the researcher would offer to verbally translate it and conduct a structured interview (using the questionnaire) at a time of the participant’s choosing. However this type of request was never made to us.
Reviewer comment: where the questionnaires translated in local languages (given that I assume that not everybody speaks/reads english, although the official language)? what about people who cannot read/write?

Authors’ response: Only an English language version of the questionnaire was provided. We made this decision based on the fact that the English language is an official language in Kenya and as such we anticipated that the large majority, if not all, of participants we would approach could understand the language. For those individuals that we invited to participate could not read or write, the lead researcher – who speaks Swahili and the two local dialects Kikisii and Kikuyu - when administrating the questionnaire was on-hand to offer support if required. All of the participants were able to understand the questions and answers in English and we received no requests from invited participants to verbally translate the questionnaire. Despite this we cannot completely rule out that some of those who refused to participate could not understand English. This point – and the implication this has for possible sample bias – has now been added to the limitations section.

Reviewer comment: how come that in one camp only 4 people were included in the study?

Authors’ response: The Minto camp only consisted of 20 tents. As stated in the sampling section, we applied an interval of approaching every 3 tents as part of our systematic sampling strategy. This meant that for this particular camp that 6 camps tents approached, 4 of which the approached individual was available and agreed to participate (see table 1).

Reviewer comment: statistical analyses are rather descriptive. is it possible to answer the research questions with these rather "basis" analyses?

Authors’ response: We believe so. The aim of the study was to investigate mental health, quality of life and life satisfaction which our analytical approach enabled. Using appropriate non-parametric statistical tests we were also able to examine which explanatory factors significantly associated (or correlated) with our outcome measures.

Reviewer comment: Results: abbreviations in the tables need to be explained

Authors’ response: Thank you. These have been now added.

Reviewer comment: Discussion: The absence of some findings, in contrast with other studies, needs to be discussed

Authors’ response: Thank you. We have now discussed this: please see lines 399-418.

Reviewer comment: More attention needs to be given to the possible implication of the study

Authors’ response: Thank you. We have made the key implications clearer across lines 443- 452.

Thank you very much to both of the reviewers for their very helpful and valuable feedback which we feel has helped to significantly strengthen our manuscript.