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## ***Interactive comment on “Living with climate change: avoiding conflict through adaptation in Malawi” by H. Jørstad and C. Webersik***

**H. Jørstad and C. Webersik**

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Thank you very much for your invaluable and extensive comments. We also address your comments line-by-line, please see below.

General comments

“The discussion paper ‘Living with Climate Change: Adapting to Environmental Change in Malawi’ in its current form is not of sufficient quality to be accepted for publication. While it contains some promising ideas and potentially interesting results it is conceptually weak, under-theorized and lacks in overall academic quality (see my detailed comments below). In sum, I would not recommend the publication of the paper in its current form. It would require a substantial effort to address the concerns I have with

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this paper. Having said that, it contains a potentially useful assessment and potentially valuable content and might be worthwhile if the authors managed to revise it accordingly.”

We have substantially revised and re-written the article with respect to your comments (Please see attached revised article in track changes).

Aim and research questions:

“The aim/objective of the paper remains somewhat unclear, mainly because I can find at least three different aims (p. 2419, line 24f): o ‘...to better understand human-environment interactions, more specifically climate change adaptation and its limitations’, OR o ‘...to what extent local communities are affected by climate change and how they adapt to it’ OR o to ‘critically evaluate[s] the long-term effectiveness and relevance of an adaptation project implemented in these communities’?, OR o assess whether the ‘LCBCCAP has significantly increased their adaptive capacity’ (p. 2432, line 12f). If the purpose is to evaluate the LCBCCAP project (which section 5 & 6 seem to suggest), the paper lacks distance to the project (see for instance the sentence starting with ‘Thus, in communities throughout the...’ (p. 2432, line 29) and more critical evaluation.”

“Also, there are no clear evaluation criteria set out by which the authors could possibly proceed with their analysis. It might be a good idea to work with research questions instead. This might also help the authors improve the overall structure of the paper (see my next comment), as they could work their way ‘through’ a number of key questions.”

We tried to clarify the aim and research questions of the paper. We decided to focus on a better understanding of human-environment interactions with reference to climate change adaptation. In order to guide the reader and to better reflect the overall purpose of the article, we introduced two research questions, one dealing with the perceptions of climate change, and how local people are affected by it, and a second addressing the effectiveness of climate change adaptation strategies in the case study area. Structure

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and focus:

“The paper lacks an overall structure. Especially section 4 and 5 seem to mix results/analysis and discussion of results. It might be worthwhile considering separating a summary of the interview responses (presentation of results) from a type of analysis/discussion (it does not matter really what label the authors want to use, but it is essential for the reader to understand where the data ends and the interpretation begins). In the current version this gets mixed up at times, e.g. the sentence on p. 2430, line 1f: ‘The changing climate is having a significant impact on the rural poor’s human security. It is pushing the people living in the Lake Chilwa Basin further into poverty by affecting the natural resources they depend on’ would for me be more of a conclusion than an analysis.”

Large sections of the paper have been re-written and edited to improve the structure, in particular to separate results (section 4 and 5) from the discussion (section 6). To do so, we have included also a scientific section on climate variability to contrast this with local perceptions of change. Moreover, and as suggested, we have made better use of the empirical material, and included direct quotes from the interview material. We also added section 7 on local knowledge, to demonstrate how adaptation can work for other vulnerable groups if local knowledge is embedded in the project design.

Methodology:

“For an empirical paper this needs a lot more attention! On page 2425f, the methods are described but these are not very detailed - more information on who, how, when, why - of those involved would provide more context. What methods were used for data collection, reduction, analysis? - E.g. how did the authors select the 18 participants, what was the focus of the interviews (specific questions or general themes)? Also: why did the authors interview members from ‘Women Fish Processing Groups’ on their agricultural practices (which seems the main focus of the results section 4)? This might make sense (for instance if this assessment is part of the broader ‘Lake Chilwa

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Basin Climate Change Adaptation Programme (LCBCCAP)'), but it needs to be explained properly. - It also remains unclear why Lake Chilwa Basin in Malawi was chosen as a case and why it is interesting. - Finally, if interviews were recorded it might be a good idea to provide some quotes throughout the results and discussion sections to showcase these key results.”

**Methodology and research ethics:** This was a weakness, and we added an entire section on methodology that also makes reference to research ethics, and includes some more information about LCBCCAP. The methods section now explains why a qualitative research approach was chosen, and the rationale for selecting the Lake Chilwa Basin. We now explain why questions on agricultural practices are important. As suggested above, we also added some quotes to the results and discussion section. We also reflect briefly on research ethics and practicalities towards the end of this section.

**Emphasis on conflict:**

“The link to conflict (a focus in the title, section 2, and which also re-appears in the conclusions), seems a bit far fetched in my opinion and it remains entirely unclear what the semi-structured interviews with the 18 women possibly reveal in relation to these issues. The way the results are presented now do not justify these linkages to be drawn.

It seems to me that either, a) the authors are trying to connect empirical material that did not have a specific focus on conflict to the broader conflict-climate discussion (which would provide a thin argument and would be highly problematic in my opinion) or, b) there has been an explicit focus on conflict in the empirical data (in which case it needs to be highlighted much clearer).”

The link to conflict or rather the missing link to conflict has also been raised by another anonymous reviewer, and we took this very seriously. We now focus the paper on adaptation and its limitations with special reference to the case study, in order to make the

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paper more coherent and to pay credit to the empirical material. Accordingly, we have changed the title. In a nutshell, we discuss what the empirical material offers and what not. This has led to substantial editing throughout the paper, with some of the sections being omitted. Some of this has also been mentioned in the specific comments sections of the review, and we have addressed these specific comments.

One could argue that due to successful adaptation measures, conflict could be avoided, however, the empirical material does not support this hypothesis. Still, this aspect could be mentioned in the context of the special issue. There is a lot of room for adaptation, for example out-migration, temporary re-location, that can ease tension, and some of these strategies have been mentioned in the article.

### Specific comments

Referees are asked to take into account all of the following aspects:

“1. Does the paper address relevant scientific questions within the scope of ESD? The paper’s scientific questions/aims remain somewhat unclear, but the paper seems to fit within the scope of ESD.

2. Does the paper present novel concepts, ideas, tools, or data? No novel concepts, ideas or tools are presented as far as I can see, but the paper does present some empirical results from Malawi that, if improved, might provide some novel insights.”

Most important, we are extremely grateful for the rigorous and diligent work of the review, addressing the specific and even technical issues of the paper. Overall, we do agree with all of the specific and technical corrections and other comments, and tried to address them to a satisfactory level.

“3. Are substantial conclusions reached? Some of the paper’s conclusions are very interesting and worthwhile – e.g. that for livelihood diversification to be an effective adaptation strategy, the additional income sources ought to not be vulnerable to the same climatic factors – but overall they remain weak (which might be due to the lack in

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overall focus and unclear research aim).”

We have focused in the conclusions on livelihood diversification as an adaptation strategy, and included a section on local knowledge to demonstrate under what circumstances climate change adaptation can work.

“4. Are the scientific methods and assumptions valid and clearly outlined? No, nor methods or underlying assumptions are discussed sufficiently in my opinion. As per my comments above, for an empirical paper the overall methodology needs a lot more attention! On page 2425f, the methods are described but these are not very detailed - more information on who, how, when, why - of those involved would provide more context. Finally, it would be great if you could use some quotes (or some specific data) throughout the results and discussion sections, showcase these key results, if possible. In addition, given that the authors academics from Europe and have interviewed 18 women from Malawi, a reflection on research ethics, informed consent and cross-cultural research practices is absolutely essential! Also: what language(s) were the interviews conducted in, was there a translator, how did you get access to the interview participants?”

We have added a methods section, and better linked the introduction and conclusions with the empirical material. We also included quotes from the empirical material, and outlined in what context, language and where the interviews took place.

“5. Are the results sufficient to support the interpretations and conclusions? No. Especially the link to social conflict (a focus in section 2 which re-appears in the conclusions), seems far fetched in my opinion and it remains entirely unclear what the semi-structured interviews with the 18 women possibly reveal in relation to these issues or the avoidance of conflict. The way the results are presented now do not justify these linkages to be drawn and I would urge the authors to be careful with making such statements.”

This point is well taken, and we have no focused the paper on the adaptation strategies

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and perceptions of climate variability.

“6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? No. As per my comment above, how the data was collected, under what conditions, with what guiding questions remains unclear.”

We hope to better explain the data collection in the methods section that had been added.

“7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? To the best of my knowledge, the authors give proper credit to other people’s work.”

“8. Does the title clearly reflect the contents of the paper? Definitely not! As per my comment above on the results, the paper does not provide sufficient empirical data to be able to make any statement about the connection between climate change, adaptation and conflict in Malawi. I would strongly suggest the authors revise the title ‘Living with climate change: avoiding conflict through adaptation in Malawi’ to something that actually reflects the studies contents!”

This point is well taken, and as a consequence, we have changed the overall title of the paper to better reflect the empirical study on climate change adaptation.

“9. Does the abstract provide a concise and complete summary? After revising the entire paper (aims, questions, structure, conclusions) the abstract will need to be rewritten.”

The abstract has been modified.

“10. Is the overall presentation well structured and clear? No. As per my overall comment above, the author lacks a clear structure and the line between results, analysis and conclusions gets very blurry at times.”

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We have now improved the structure, and hopefully made the distinction between results (section 5) and discussion (section 6 and 7) more clear.

“11. Is the language fluent and precise? The language is OK.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes.”

“13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes. P. 2421, entire section ‘2.2 Climate change adaptation’: there have been gazillions of summaries about the development of the concept of ‘climate adaptation’ so I think the first four paragraphs can be entirely deleted or at least collapsed into a few (!) sentences.

We have reduced the text of 2.2 substantially.

“Table 1 seems redundant, I suggest deleting it.

Yes, we agree, the former table 1 was deleted.

“Figure 1: What do the numbers from 0-10 on the vertical axis represent? The numbers of respondents for each impact? Needs clarification!

We clarified former figure 1, now figure 2.

“Figure 3: Unclear how this figure came about: What was the question posed to the 18 respondents? Was it an open or closed question? Where the answers pre-given (i.e. could respondents only choose between these two) or did the authors develop these three (very generic) categories based on what respondents talked about? Requires more explanation.”

Text has been added to better explain former figure 3, now figure 4.

14. Are the number and quality of references appropriate? Generally the paper would benefit from more references (I have made concrete comments in the ‘Specific com-

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ments' section below.)

We have added more references to the text, where appropriate.

15. Is the amount and quality of supplementary material appropriate? Figure 3 does not contain much information at present, but might be useful once revised (see my comments in response to question 13)

Technical corrections and other comments

- P. 2418, line 4&5, also line 15: The way the authors discuss the 'climate sensitive economy' of developing countries excludes any discussion of differential vulnerability to existing inequalities and historical injustices. People are not simply climate vulnerable or have 'low adaptive capacity' because they rely on agriculture, but because there are many social, economic, political etc. factors that render them vulnerable in the first place. There is a breadth of academic literature on this, which should be at least be noted here in my opinion.

Yes, we have mentioned the concept of differential vulnerability in the 1.

- P. 2418, line 19-21: 'While the debate on climate change and violent conflict remains inconclusive, new research on linking climate change to human security seems to be more promising'. In what sense 'promising'? Promising of what, for what?

We have omitted this sentence.

- P. 2418, line 26f: Sentence starting with 'Climate change adaptation ...'. This is a bit of a weird sentence. First of all, adaptation is not only highly relevant in a 'developing country context', but also for 'developed countries' (think of London, of the Netherlands). Second, how does the limited responsibility for GHG emissions create a greater need for adaptation? I suggest revising this sentence.

We revised this sentence.

- P. 2419, line 5: Suddenly the authors speak of 'low- and middle-income countries'.

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This contrasts somewhat with p. 2418, line 26f (also see my previous comment). Needs revising.

Yes, we agree, we have changed this.

- P. 2419: What do you mean by ‘climate-centric research’? I think I do understand what the authors mean, but this needs explanation! Do you mean positivistic, deterministic, reductionist, causal?

Yes, this is what we mean but for the sake of clarity and new structure, we have omitted this part.

- P. 2419, line 12: Sentence starting with ‘Though the authors introduce control variables ...’. My question to you: To what extent can you possibly ‘model’ human behavior across cultural communities? This is a huge fault of research built on positivistic assumptions (common for instance in Actor Based Modeling approaches) which has been criticized widely e.g. by sociologists, anthropologists, human geographers. This needs to be reflected here if your aim is to discuss ‘What is missing from this analysis is to put climate change impacts and its social consequences into context’.

Yes, we agree, though we have omitted this discussion as it does not centre around climate change adaptation and its limitations.

- P. 2420, line 3f: How do you ‘evaluate’ the linkages? By what criteria?

Not applicable as this part has been removed.

- P. 2420, line 13f: ‘As the introduction illustrates, Malawi meets the dominant indicators used by typical neo-Malthusian resource scarcity-conflict studies’. The introduction says nothing about Malawi (as a country), nor does it discuss neo-Malthusian studies (this follows in the paragraphs after this sentence). Perhaps this is a remnant sentence of an earlier version of the paper? I suggest revising this sentence or moving it to a part of the paper where it makes sense.

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We removed this sentence and the discussion on neo-Malthusian resource scarcity-conflict studies to better reflect the empirical case study on climate change adaptation.

- P. 2420, line 16 and line 18: 'Malawi is yet to see any major armed political conflicts' AND 'Sustainable adaptation strategies can therefore be seen as a means to avoid conflict situations': these two sentences suggests the authors buy into the simplistic/reductionist/deterministic/causal scarcity=conflict narrative. From the introduction I got the impression they were criticizing this perspective. . . ? I am confused and suggest a clarification is needed here.

Yes, this claim cannot be supported with the empirical material, and therefore has been omitted from the paper.

- P. 2420, line 20: 'Much of the literature on climate change conflict has been dominated by neo- Malthusian ideas, emphasising a deterministic view of linking population pressures and resource scarcity to undesirable outcomes'. This is nothing new (i.e. the authors invention), nor is it too common of knowledge. Hence this sentence needs references.

This sentence has been omitted.

- P. 2421, line 1: Sentence starting with 'One explanation for Malawi's peaceful pathway . . . '. Is this an assumption or one of your conclusions you made after the analysis? To me it sounds more like the latter.

This also has been omitted.

- P. 2421, entire section '2.2 Climate change adaptation': there have been gazillions of summaries about the development of the concept of 'climate adaptation' so I think the first four paragraphs can be entirely deleted or at least collapsed into a few (!) sentences.

We have significantly reduced text and collapsed text into few sentences here.

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- P. 2422, line 23: 'These adaptation strategies are considered to be relevant also for Malawi.' By who? The IPCC or the authors? If the latter, that needs to be clarified, if the latter, it would be a conclusions the authors can make after having done their analysis. If so, I'd suggest to rewrite the sentence into something like 'The results of this paper suggest that these strategies can also be considered relevant in the context of Malawi'.

Sentence has been re-written.

- P. 2422, line 26f: Whether 'adapting to climate change may require human migration and resettlement' is a widely contested and hugely political issue. This sentence needs references! The authors may also want to consider reflecting a bit more on this.

We very much agree and have added a couple of sentences to reflect this discussion.

- P. 2424, line 8-12: This whole paragraph is missing references. Unless the authors have made all these assessments themselves (which would of course need to be reflected a revised methodology section) they are needed here.

We have added the source for this.

- P. 2424, line 22: Sentence starting with 'It must be noted that there is still no consensus ...'. The word 'consensus' would suggest some kind of disagreement or at least differing accounts. If so, it would be good to briefly explain what that entails and who (e.g. between authors, organizations?).

We have deleted this sentence.

- P. 2425, line 26f (running over to the next page): Is this paragraph about the paper's methodology or that of the referenced LCBCCAP/WF- PGs programme. If the latter, I suggest deleting the entire paragraph. If the former, this would need to come much earlier in the paper (for instance on page 2420) between the aims and the overview of the paper.

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This part has now been integrated into the new methodology section.

- P. 2426, line 9: 'For many, climate change is something that belongs to the future', generic sentence. I suggest deleting.

Yes, this sentence has been deleted.

- P. 2426, line 11f: Again, this is confusing: 'The study found . . .' is this someone else's study or the paper's authors study? – It remains unclear why section '4.1 Climate change in the Lake Chilwa Basin: local experiences' and section '4.2 Impacts of climate change: local perspectives' are separated. The content seems very similar.

Where appropriate, we added the qualifier "authors" to make clear which study is meant.

- P. 2428, line 12: Suddenly you talk about the 'rural poor in Malawi'. You may want to explain who they are, what differentiates them from non-poor rural populations and poor urban populations.

Good point. We focused now on smallholder farmers rather than rural poor to make the focus on agriculture more clear.

- P. 2428, line 13: Sentence starting with 'In Africa . . .'. I have two problems with this formulation: First, it generalizes 'Africa' thereby ignoring the significant cultural and agricultural diversity that exists across this gigantic continent. Second, it presupposes that farmers outside of 'Africa' do not rely on their local knowledge for agriculture. I think both assumptions are wrong and this sentence should be revised.

Point well taken, we have removed the word 'Africa' and instead use the word 'Malawi' to better reflect the local context.

- P. 2429, line 4f: 'The women had however been able to increase their income and savings substantially through the WFPG and were therefore capable of doing so.' If the interviewees have increasing access to income from other work through the WFPG, it

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could also be the case that they decided not to continue with subsistence farming as previously. Might be worthwhile to reflect on this here?

Yes, we added a sentence to clarify the impact of women's decision to stop farming.

- P. 2430, line 20: 'The case study found that the members of the WFPG were satisfied with their involvement in the LCBCCAP programme'. This conclusion cannot be drawn from reading the above sections (4.4.1, 4.2). If you want to discuss this, I suggest you include responses by the 18 members to back this claim.

We put this sentence into context to better explain why members of the WFPG are satisfied.

- P. 2431, line 20: 'This is a concern that also Chiotha is worried about in the Lake Chilwa district (Ngozo, 2012)'. Who is Chiotha? Is that the given name of the referenced author?

Chiotha is a member of the the LCBCCAP programme.

- P. 2433, line 4f: The authors state that 'The majority of the women however, were not diversifying their livelihood strategies', but I thought all 18 women engaged in subsistence agriculture and fish processing – isn't this a diversification?

Yes, this is diversification but not sufficient to offset the loss from the fish sector.

- P. 2435, line 2: As I understand the study looked at one adaptation project. It is unclear to me how the authors can make a statement about 'policy makers' (this might make sense once the authors have provided more background to the LCBCCAP project).

Yes, we agree and have deleted the last sentence of the conclusions.

- Line 18: delete 'prominent', this sounds like the authors are advertising. I suggest deleting this word.

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Word has been deleted.

- P. 2424, line 6f: Missing word, insert 'of Malawi's population' after 74% as in '74 % of Malawi's population live on less than a dollar (PPP) a day (2004, estimate) (UNSTATS, 2012).

Added missing word.

- P. 2425, line 22: Missing word, insert 'a', as in 'Findings from a case study of the Lake Chilwa Basin Climate Change Adaptation Programme (LCBCCAP) and its Women Fish Processing Groups (WF- PGs), revealed . . . '

Yes, done.

- P. 2432, line 20f: It should read 'Nelson et al. (2009) explain', not 'explains'.

Yes, done.

We hope you find our review satisfactory, and look forward to further additions and comments. Once again, your review is greatly appreciated and has substantially improved the paper. To follow the changes, the revised article in track changes is attached.

Please also note the supplement to this comment:

<http://www.earth-syst-dynam-discuss.net/6/C1324/2016/esdd-6-C1324-2016-supplement.pdf>

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Interactive comment on Earth Syst. Dynam. Discuss., 6, 2417, 2015.

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