

Weather Clim. Dynam. Discuss., referee comment RC1
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Comment on wcd-2022-49

Anonymous Referee #1

Referee comment on "Effects on early monsoon rainfall in West Africa due to recent deforestation in a convection-permitting ensemble" by Julia Crook et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2022-49-RC1>, 2022

Review of manuscript # **wcd-2022-49** titled 'Effects on Early Monsoon Rainfall in West Africa due to Recent Deforestation in a Convection-permitting Ensemble'

submitted to EGU Weather and Climate Dynamics – Discussions

Submitted by – Julia A. Crook et al. 2022

The study pertains to understanding how current west African deforestation impacts the regional hydroclimate as compared to the land cover in the 1950s using a cloud resolving regional atmospheric model. The pre- to early- monsoon period is simulated and various hydro-meteorological variables are analysed to understand the impacts of deforestation of convection, precipitation and underlying processes. The authors show small but significant increases in precipitation due to deforestation unlike the findings of a precipitation decrease by previous studies, which is the novelty of this result and probably also matches with some observations as claimed by the authors. It seems that the use of the cloud resolving simulations have helped the authors to achieve these similar-to-observation results. Overall I think the study and its results are important for publication. However, I do find certain issues with the writing and presentation style which have made the article

a difficult read. There are also certain aspects of the study that need clarification in the paper. I suggest a revision of these aspects of the study before final acceptance. I have provided my specific comments below. I have also provided my answers to the questions provided on the journal website -

- Does the paper address relevant scientific questions within the scope of WCD? Yes
- Does the paper present novel concepts, ideas, tools, or data? No
- Are substantial conclusions reached? Yes
- Are the scientific methods and assumptions valid and clearly outlined? No
- Are the results sufficient to support the interpretations and conclusions? Yes
- Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? To some extent
- Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes
- Does the title clearly reflect the contents of the paper? Yes
- Does the abstract provide a concise and complete summary? Yes
- Is the overall presentation well structured and clear? No
- Is the language fluent and precise? No
- Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Almost
- Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes
- Are the number and quality of references appropriate? Yes
- Is the amount and quality of supplementary material appropriate? Yes

Specific Reviewer Comments –

- The writing style is confusing and not succinct. At some places important information/explanation seems to be missing. At some times it even sounds more casual than expected for a scientific article. Examples – section 2.1.3, paragraph 1 of section 3.6. Some of the results need to be presented in a better way as well. For example, I found section 3.6 quite difficult to understand in the first reading probably

because the results and discussion have not been presented clearly. It is hard to point at more examples, and ways in which improvements can be made, but there is nevertheless a general need for improvement on the writing style. A revision would help make the results more accessible to readers.

- A more detailed discussion of the cloud resolving model is needed. A 4 km spatial resolution falls in the grey area where clouds cannot be simulated explicitly and cannot be parameterized properly because the assumptions of regular cloud parameterizations, based on spatial statistics applicable to larger scales break. So the authors should explain in more detail why they have chosen to work in this intermediate spatial resolution.
- Some evaluation of model results with observations is needed. While there might not be in-situ observations available from this region, there are satellite data products of cloud and precipitation (on larger spatial scales) available which can be used to at least provide a qualitative comparison with observations. The authors have also mentioned some previous observational studies and that their results relating with precipitation changes agree with these observations. For example on Line 323 authors have referred to Taylor et al. 2021. It would be better if some of these observations are included in this manuscript to (1) validate the baseline simulations and (2) provide comparison to simulated changes.
- Why have the authors not analysed the changes over regions like Guinea, Sierra Leone, Cameroon and central African Republic where the change in tree cover is the maximum? Although the authors have presented some valid reasons for their choice of analysing the two regions, it is still unclear why they chose these regions over some other very interesting regions in their simulated domain where interesting and larger changes to the hydro-climate have occurred. While there might be an obvious reason for this, it is not clear in the writing. Authors should point that out more clearly.
- Line 205 – it would be helpful to mention here itself which two deforested regions have been analysed.
- Line 285 – there should be more discussion around what the delta theta proxy means. The conclusions from this analysis are also not stated clearly. The usage of this proxy in the following sections is also not effective because the meaning of this proxy and implications of its change are not clearly defined.
- Figure 1 and associated text under section 2.2 can go to Supplementary Information.