Weather Clim. Dynam. Discuss., referee comment RC2
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Comment on wcd-2022-49

Anonymous Referee #2

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-RC2, 2022

The study applies a 5-day ensemble forecast over West-Africa using a convection permitting regional climate model in order to understand the effects of historical deforestation within the region. Due to the high spatial and temporal resolution of the climate model data the authors are able to dissect the processes controlling the weather response within West-Africa as a whole and within 2 subregions highlighting how different local conditions can alter the response to deforestation. Overall this is a unique approach within the research on effects of land cover change on climate and it opens a lot of interesting questions worth exploring in future studies. The study is well structured and written clearly and in a comprehensive way as it addresses several variables in order to understand the changes physically. I would recommend the journal to accept this paper after addressing some minor specific questions added here below.

- Research on deforestation in global idealised simulations studies (a.o. Winckler et al., 2017) have tried to separate local (mostly roughness and albedo effects) from remote effects (large scale circulation), as you highlight in line 109 this study mainly focusses on the local effects and the short period of simulation time does not allow (large-scale) circulation aspects to occur and to influence the results. This might be a strong assumption as these large scale effects strongly influence several variables focussed on within the study (e.g. rainfall due to shifts in ITCZ, Devaraju et al.,2015). How important do you think this bias would be for the interpretation of the results? Do you think that the lack of these large scale circulation changes could help explain the differences between your results and the 3 studies compared to in section 4? I feel these aspects although flagged at some points are not fully addressed yet.
- I'm intrigued by the approach of a 5 days forecasting ensemble, as far as I am aware this has not been used to asses effects of land cover changes which adds a strong novelty to the study. However I wonder how generalisable these results are? You highlight the importance of choice of season and month in several locations within the manuscript, but wouldn't some effects have a delay of occurring (e.g. initial wettening due to deforestation but after while drying?). This is a known caveat of the method I presume, but I wonder if this could be overcome by for example running this ensemble longer (eg 30 days)? For clarity I do not request additional simulation, but I think some

discussion on these methodological aspects would be interesting to include.

- Due to the unique setup of the study it opens a lot of questions for future research of which you highlight some in section 5. Could you go a bit further and try to give some recommendations for example : How can this study inform future work by earth system models and regional climate models? What would be priorities for development or research based on this work, should models invest in more convection permitting and/ or deeper evaluation and developments of surface scheme? I believe these kind of insights can help guide the model development community greatly. Therefore I would suggest to include something in line of a limitations and outlook section within the manuscript in order to have a general discussion on the implications and weaknesses of this study now some of these aspects are mentioned in the conclusions but I feel you could go further in this discussion.

Technical corrections:

line 49-52: There is a useful review by Perugini etal (2017) who also have a similar conclusion

line126-127: The LUCID studies by Pitman et al. 2009 indeed show this but more recently also Boysen et al. 2020 showed in the LUMIP deforest_glob runs that there still remains large issues and uncertainty within ESMs.

Line 260: More recently Duveiller et al 2020 (https://doi.org/10.1016/j.landusepol.2019.104382) have a more comprehensive dataset of near surface temperature using similar approaches as Alkama and Cescatti, 2016

Figure 9 and 11: I found it a bit unclear what all the lines were indicating on the plots (I initially overlooked the different colours of the labels) perhaps this is my own fault but to help people like me I would suggest to add the colour of the lines between brackets after the variable is introduced in the subscript (e.g. (a) number of spontaneous initiations (green) and number of storms present (blue)). Additionally I find the colours of the last panels (c in Figure 9 and c and f in Figure 11) very similar between nstorms and intensity, I would suggest to change it to a more different colour.