

Interactive comment on “Projected effects of vegetation feedback on drought characteristics of West Africa using a coupled regional land–vegetation–climate model” by Muhammad Shafqat Mehboob et al.

Anonymous Referee #2

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1. Brief summary of the manuscript

In their manuscript, Dr. Mehboob and co-workers applied a regional climate model coupled to a dynamic vegetation module to quantify the effects of vegetation feedback on drought over West (Sahel and Gulf of Guinea) and Central Africa (Congo Basin) under present-day and future climate. To identify drought conditions, the authors use the Standardized Precipitation Evapotranspiration Index (SPEI) as defined by Vicente-Serrano et al. (2010) by combining monthly precipitation and potential evapotranspiration (PET). To assess the added value of representing the dynamics of vegetation

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processes (e.g., plant shift, growth), Mehboob et al. performed numerical experiments with and without the dynamic vegetation module. In addition, they accounted for uncertainties in the atmospheric forcing by taking boundary lateral conditions from four global climate models (GCMs). The main results are:

- In experiments using the dynamic vegetation module, future drought lengthens and strengthens in the Sahel compared to experiments without the dynamic vegetation module, while the trend is less clear in the Gulf of Guinea and the Congo Basin.
- When forcing the regional climate model with different GCMs, results are consistent except for the Congo Basin where GCM diverge in reproducing drought frequency under present-day and future climate.

2. General comments

The study addresses relevant scientific questions that are within the scope of HESS and that are related to drought occurrence and intensity in a sensitive region such as West and Central Africa. In this sense, the study could provide interesting advance towards current knowledge and methodologies applied to project drought in Africa and other sensitive regions using RCMs. However, in my opinion, the quality of presentation is poor and confused; the Introduction, Methodology, and Results and Discussion Sections are not well laid out; some methodological choices are not well justified; and the significance of results is not discussed. Moreover, I would suggest to edit and proof-read the manuscript to avoid redundancy and to simplify some confused sentences that make the reading difficult. In the following, I provide specific comments (major and minor) on the manuscript.

3. Major comments

In my opinion, the **Introduction** does not provide enough information to readers on the

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target region, its climate features (also in terms of surface-atmosphere interactions) and on the vegetation feedback the manuscript will focus on. Although the authors cite some previous works that studied the same region, I think the authors should spend more words in summarizing the main results and limits of the cited works. This will allow the authors to clearly state their own original contribution to the tackled topic.

LI 31 (pag. 2): "... on a balanced emphasis on all energy resources...": It is not clear to me what this mean. I suggest to rephrase this sentence and describe more explicitly the methodology of the cited work of Caminade and Terray (2010).

LI 36 (pag. 2): For sake of completeness, I would mention that RCM can be forced using re-analysis

LI 45–48 (pag. 2): I think it would be interesting to summarize the main findings of the study of Cook and Vizy (2008), in particular the effects on the regional climate of South America of a reduction of 70

bf LI 53 (pag. 2): "...climate draft...": Again, this expression is unclear to me, I suggest to express this differently.

LI 55–63 (pag. 2): In my opinion, it is not clear why the authors have chosen the SPEI instead of other drought indexes. I would suggest to present the advantages and the limits of using the SPEI to identify and project drought.

In the **Methodology** section, I think the description of the dynamic vegetation module and its functioning should be more detailed. Moreover, I do not understand which parameterization scheme the authors have chosen to represent convection. Related to this point, to ensure the traceability of results, a summary table with all the selected parameterizations could be useful for readers that would like to apply the same modelling set-up over a different region.

In terms of run experiments, in my opinion, the study lacks an experiment forced by re-analysis; this extra-experiment would provide a better term of comparison against observations to identify the model biases.

Regarding the SPEI index, I think its computation should be described in a clearer

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way. For example, the Thornthwaite method should be presented in more details to allow the readers to understand how the potential evapotranspiration is derived. Specifically, this method should also be shortly reviewed in comparison to other well-known methods (e.g., the Penman- Monteith equation), in a more detailed way than that reported on page 7 (ll. 7–11). Lastly, in the manuscript, the authors refer to drought frequency. However, it seems to me that they did not explicitly define how drought frequency has been calculated.

LI 82 (pag. 3): "... aN ordered data structure ...", it is not clear to me what this refers to. I would suggest to make this explanation more explicit.

In my opinion, in the **Results and Discussions** section, the model evaluation should be performed using a simulation forced by re-analyses. In the model evaluation presented in the manuscript, it is difficult to understand how the divergent behavior of GCMs over the Congo Basin may influence the ensemble mean, which is compared to observations in Figure 2. In general, I found the presentation and discussion of results confused and hard to follow using the provided figures. My suggestion would be to (a) re-structure this section and the related figures, (b) include a more quantitative discussion in relation to other studies, and (c) asses the significance of the shown results.

LI. 15 (pag. 4): "... different RCMs ...", by checking the study of Erfanian et al. (2016), I think the authors are referring to different GCMs.

LI. 18 (pag. 4): "... overestimating precipitation ...", it is hard to compare the figures and to distinguish the differences between observations and simulations, however it seems to me that precipitations are under-estimated over the Gulf of Guinea and the Congo Basin. A plot showing the differences between observations and model experiments will ease the identification and interpretation of model bias.

LI. 25–26 (pag. 5): This sentence is not clear to me. In RCM experiments, the climate forcing is prescribed, hence I do not understand how "a change in vegetation could

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impact climate forcings".

LI. 45–46 (pag. 5): It is not clear to me that the experiments using the dynamic vegetation module clearly capture the "more severe and longer droughts". I think to support this statement an observation-based SPEI would be needed. If the authors could compute SPEI based on observations, I would suggest to add a line in Figure 6 that shows the monthly observation-based SPEI.

LI. 35 (pag. 5): " (Fig. 2c-3)" It is not clear to me if the authors are referring to Figure 2c and the whole Figure 3 or to something else.

In my opinion, the **figures** are not well laid out because title and units are only inserted in the figure caption. Since all the figures are multi-panel, the reading becomes even more complex. Moreover, in Figure 1 the three boxes are nearly invisible. I would suggest to highlight better the three target regions and to draw these boxes on all the maps that are presented in the study.

4. Minor comments

Below, I list typos and errors, and I point to sentences that I would suggest to rephrase in a clearer way.

LL 14–15 (pag. 1): I would suggest to replace "With utilizing ..." with "Using ..."

LL 16–17 (pag. 1): I would suggest to replace "With the vegetation dynamics ..." with "By considering vegetation dynamics ..."

LL 33 (pag. 2): "... that western end of Sahel ... whereas eastern Sahel..." should be replaced with "...that the western end of Sahel ... whereas the eastern Sahel ..."

LL 36 (pag. 2): I would suggest to remove the comma between "... remain ..." and "... because ..."

LL 42 (pag. 2): "... variability, he claimed ..." should be replaced with "... variability; the authors claimed ..."

LL 43 (pag. 2): "Various studies ... have been documented ..." should be replaced with

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"... Various studies documented biosphere-atmosphere interactions ..."

LL 51–54 (pag. 2): I would suggest to rephrase these two sentences to make them clearer and avoid redundancy.

LL 55 (pag. 2): "...Draught ..." should be replaced with "... Drought ..."

LL 57 (pag. 2): "..., which ..." should be replaced with "... that ..."

LL 79 (pag. 3): A space is missing before "Cloud"

LL 81 (pag. 3): I would suggest to correct and simplify this expression: " While solving a surface biogeochemical, biogeophysical, ecosystem dynamical and hydrological processes ..."

LL 88 (pag. 3): "... distribution and vegetation distribution ... is established ..." should be replaced with "... distribution and vegetation distribution ... are established ... "

LL 91–93 (pag. 3): I would suggest to rephrase the sentences that describe the different simulations to make them clearer and avoid redundancy.

LI 05 (pag. 4): The acronym PET has not been previously introduced.

LI. 56 (pag. 5): "CO2" should be replaced with "CO₂"

LI. 75 (pag. 6): The comma between "ensembles" and "show" should be removed because it divides the subject from the verb.

LI. 35 (pag. 7): "... CCSM show somewhat ..." should be replaced with "... CCSM shows somewhat ..."

LI. 08 (pag. 7): There is an extra "that" which needs to be removed

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