Reply to Anonymous Referee #1 on the manuscript
"Contrasting dynamics of hydrological processes in the Volta River basin under global warming"
(Manuscript hess-2021-525)
by M. Dembélé et al.

I enjoyed reading this manuscript. The authors used a distributed model for assessing climate change impacts on different fluxes and discharge output in Volta basin. The ms is quite elaborated and fits well with HESS standards. I only have several concerns regarding "dynamics" and "uncertainty" results presented in the manuscript. Moreover, climate gradient in the basin seems to ruin (dominate) AET patterns censoring vegetation dynamics.

We thank the reviewer for this very positive overall appreciation of our work and the constructive review and valuable comments. Below are our responses to each of the comments.

Specific comments:

-Title is very catch but I couldn't find much on "dynamics" presented in the results except for the Fig5, 13 and 14.

The term "dynamics" is used here to refer to the temporal and spatial changes observed in the hydrological processes as a result of climate change. In addition to Figures 5, 13 and 14, Figures 6, 7, 9 show the temporal dynamics and Figures 10 and 11 show the spatial dynamics of hydrological variables. Moreover, additional figures are available in the supplementary material to support our analyses on the dynamics of hydrological variables (e.g. Figure S1 to Figure S30). However, we will do further analyses on the seasonality as suggested by the reviewer below.

-In addition to Fig13, the readers would be curious to see Lahaa and Blösch (2006) type seasonality figures for regime (a kind of dynamics) changes/shifts in the basin. The seasonality indices could be adopted to low and high flows as done in different other papers below. Event definition is key here for counting them i.e. Q95 and Q5 for low and high flows.

-Seasonality shift is only mentioned for rainfall at line 491 in conclusions but there is room for assessing shifts in high and low flow occurrence (dynamics) and seasonality.

Figures 4-6-9 in Laaha and Blösch (2006) DOI: 10.1002/hyp.6161 are good examples.

Similar applications in climate research:

https://mdpi-res.com/d_attachment/water/water-12-03575/article_deploy/water-12-03575-v3.pdf

https://mdpi-res.com/d_attachment/water/water-11-00925/article_deploy/water-11-00925-v2.pdf

We thank the reviewer for sharing approaches to investigate shift in high and low flows. We will investigate the method of circular statistics adopted in the suggested papers to assess changes in the days of occurrence of low and high flows and their variability, which is an indicator of seasonality.

-Fig 11, 12, S38, S49 (AET in particular) are mostly dominated by climate gradient and not showing vegetation dynamics. The authors should find a way to exclude the dominant effect of rainfall using a normalization procedure. A new procedure is proposed in this paper

Example:

https://www.preprints.org/manuscript/202111.0225/v1

However, there must be other methods approaches in the literature for deblurred AET pattern maps by removing climate gradient.

https://www.researchgate.net/publication/338208138_Image_Deblurring_Techniques_-A_Detail_Review

We agree with the reviewer that actual evaporation (Ea) patterns are governed by rainfall patterns in the Volta basin. The study area is located in a semi-arid zone where the main driver of the water cycle is rainfall, with annual Ea accounting for 80% of rainfall. Therefore, Ea naturally follows the patterns of rainfall, which can also be observed for vegetation, i.e. there is more vegetation in the southern part of the basin where rainfall is high and less as we move towards the north (Figure 1). The normalization of Ea would not bring substantial information to the reader as we are interested in analyzing the main hydrological variables and not their derivatives (e.g. the evaporative index, calculated as the ratio of Ea to P). We understand the potential value of the normalization for model calibration but the goal of our manuscript is not focused on calibration, which we did in a previous study (https://doi.org/10.1029/2019wr026085)

-Line 196: "Uncertainties in the model inputs and outputs are assessed in terms of variability be"

Indicating V2 estimation (or even COV coefficient of variation) as uncertainty assessment is quite ambitious without a systematic uncertainty propagation like in GLUE by Keith Beven.

In short, this vague sentence should be revised as smth like "variability in the model inputs and outputs are assessed using V2 statistics".

The sentence will be revised to avoid confusion.