

Earth Syst. Dynam. Discuss., referee comment RC1  
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## Comment on esd-2021-46

Anonymous Referee #1

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Referee comment on "Weakened impact of the Atlantic Niño on the future equatorial Atlantic and Guinean Coast rainfall" by Koffi Worou et al., Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2021-46-RC1>, 2021

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In my view, the paper is within the scope of ESD and presents some interesting new findings, however, there are some questions that I would like to authors to address to improve the presentation.

Abstract: I feel that it is too long and would suggest that you shorten it. For instance, discussions of the Saharan Low can go and the discussions of the Bjerknes feedback more focused.

L.20: I don't understand how upper-level subsidence leads to the little dry season.

L.25: Does the little dry season occur in AMJ? One should expect this to correspond to the period between the double rainfall peaks?

L.40: Jouanno et al 2017 is just one of the numerous papers that discussed the Bjerknes feedback, so I don't understand why this paper is presented as the final say. I think the point is that several processes drive the Atlantic Niño variability. The next paragraph, the upper-level links to the Indian and Pacific Oceans need to be better explained.

L.50: What destructive interference means or how it operates is not clear at all.

L.55: I don't think that the discussion "...temperature and precipitation over the globe" is necessary here. I'd suggest that you remove that and keep the flow focused on the equatorial Atlantic.

L.100: Is it "realistic" or observed natural and anthropogenic forcing?

L.105: "These latter simulations..." Do you mean SSP-85?

L.115: Please use one rainfall, SST etc data to compare with the models. Comparing multiple observations is unnecessary and it makes following your discussions difficult.

L.130: Why do you use quadratic detrending, are the trends quadratic? I ask because we are more used to linear trends. More explanation is needed here.

Table 2: I don't find the numerous acronyms here very useful and they can as well cause

more confusion, given that we have the model names to deal with. AtI3 is widely known as the SST anomalies (well, or some other quantities) averaged in the Atlantic Niño region defined as 0-20W, 3N-3S; so it's not necessary to introduce a new definition ATL3B. Why define TAB1, TAB2 when there are well known regions like AtI4, tropical North Atlantic (TNA) and tropical South Atlantic (TSA)?

Data and methods section: For easy navigation, I would suggest splitting this section, for instance "Data", "CMIP6 Models", "Analysis strategy" or something similar

section 3: I don't find the line plots and discussions of bimodal structure and annual cycle necessary and I suggest that you remove these. I consider the question of annual cycles and seasonality as a separate question. Since this study is about JAS, it is enough to briefly describe the JAS patterns and biases and move to the Atlantic Niño related SST and rainfall and their future changes.

Fig. 2: How do you have strong easterly wind biases over warm SST biases? This pattern needs explanation because it is inconsistent with expectation and inconsistent with Richter and Tokinaga 2020 (see their Fig. 2).

Section 4: You are basically evaluating SST and rainfall patterns rather than "teleconnections". Secondly, regression maps show rainfall, SST etc with units in mm/day, degC etc and I don't see the need for the repeated use of regression coefficient, instead of referring to rainfall, SST etc.

L.225: Again, I don't see the need to compare observations with other observations here. I suggest that the authors rather use just one observational data to compare the CMIP models.

Fig. 4: I suggest separating this Figure so that the maps stand as one Figure, and the Taylor diagram stands as a different Figure. In the Taylor diagram, the REF which is here ERA5 should correspond to a standard deviation of 1. The authors should explain why/how their scaling leads to a different value. Again I suggest that the authors discuss the overall model fidelity using using both pattern correlations and variance (that is closeness to REF).

Fig. 5: Again I suggest two different figures: one for the maps and the other for the Taylor diagram. One satellite rainfall data and one SST data should do, no need to compare different observations which I consider outside the scope of this manuscript.

L.260: It'll be good to state what sea surface heights represent, what understanding you'll like to gain by analyzing that. The same could be said of the atmospheric variables. The motivation and physical reasoning behind the analysis need to be better formulation. For instance, SSH- $\square$ SST(atI3) regression implying thermocline impact on the SSTs which is one element of the Bjerknes feedback (Keenlyside and Latif, 2007). Then the winds/SST regressions another element? Are the rainfall and divergence related to the ITCZ/atmospheric component of the Bjerknes feedback (Nnamchi et al. 2021)?

L.310: How you calculated the percentages should be explained in context here so that it's understood what minus percentages, plus percentages mean.

L.315: This point needs more discussions/explanations of why your result is different from Brierley and Wainer (2018), Is it because the use different time slices, methods, models etc?

L.360: It's important to first outline the elements of the Bjerknes feedback as the basis for your analysis and then build the subsequent discussions around that.

L.370: I think that this paper is about weakening equatorial Atlantic variability rather than teleconnections. Please note that equatorial Atlantic doesn't mean the same thing as tropical Atlantic.

Fig. 11: I don't really find the discussions of Saharan Low interesting at all because I feel the an equatorial region is enough to interpret the results here. The Sahara/Sahel matter is a different topic.

## References

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