Answers to the Reviewer 1

We would like to thank the Reviewer for his very constructive comments and suggestions. Please, find below our response to your comments, on a point by point basis. Your comments are recalled in red and our responses are written in purple.

RC1: 'Comment on esd-2021-46', Anonymous Referee #1, 14 Sep 2021

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.

We will shorten the abstract by removing the discussion about the Saharan Low

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

A rapid shift of the rainfall belt from the coastal regions of West-Africa to the Sahel occurs around the 21st of June, and marks the onset of the monsoon season. In July and August, the development of the Atlantic cold tongue is at its maximum (due to upwelling over the ATL3 area), which leads to higher pressure over the Gulf of Guinea and the coastal regions of West Africa. As a consequence, during July and August, the atmospheric circulation above Guinea Coast is characterized by a downward flow at upper levels which reduces air ascent over Guinea Coast and leads to the so call "little dry season" (see also section 2.1.6.2 The Little Dry Season of the book: Meteorology of Tropical West Africa, The Forecasters' Handbook, 2017). This section will be rewritten accordingly.

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

The little dry season occurs every year in July and August, and indeed it corresponds 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.

We agree that many papers discussed the Bjerknes feedback. There are also many studies pointing out different forcings on the development of an Atlantic Niño event, as you stated and this is mentioned in the paragraph including this sentence. In this last sentence, we would like to present the paper of Jouanno et al 2017 as an example of a study which disentangled the relative contributions of ocean dynamics and the thermodynamic processes in the control of the Atlantic Niño/Niña development (and not as a final say). We propose to rewrite this last sentence of the line 40 to clarify this point:

“ In order to quantify the relative contributions of the different processes driving the Atlantic Niño variability, Jouanno et al 2017 highlighted the dominant role of the dynamical forcing (i.e the Bjerknes feedback) relative to the thermodynamic processes (i.e air-sea heat flux exchanges). They argued that biases in the atmospheric components of most of the GCMs participating in the CMIP project lead to the underestimation of the dynamic part of the Atlantic Niño forcings”.

Second point: improvement of the upper-level links to the Indian and Pacific Oceans.

We propose to add additional information:

The general response of the atmosphere to Atlantic Niño positive phases is a modification of the Walker circulation, characterized by rising motion and upper-level divergence in the Atlantic region and compensating upper-level convergence and sinking motion in the central Pacific that also triggers a Gill-type response in vorticity. The Gill-type response is characterized by a pair of upper-level anticyclones to the west and a pair of upper-level cyclones to the east of the abnormal warm oceanic regions. These upper-level divergence and vorticity responses are related to each other by Sverdrup balance (Hamouda and Kucharski, 2019, Losada et al., 2010, Kucharski et al., 2009). The divergence and vorticity responses are generally baroclinic, and are of opposite sign at low levels, meaning in the Indian region a low-level anticyclone is present that leads to reduced Indian Monsoon rainfall (Kucharski et al. 2007, 2008, 2009). On the other hand, the sinking motion in the central Pacific can lead to easterly surface winds in the central-western Pacific that could potentially lead to a La-Niña event (Rodriguez-Fonseca et al, 2009). Finally, the development of a La-Niña event due to a warm phase of the Atlantic Niño would favor positive rainfall anomalies over the Indian Peninsula, which would counteract the negative rainfall anomalies associated with the Atlantic Niño (Ding at al, 2012). ”

Ding, H., Keenlyside, N.S. & Latif, M. Impact of the Equatorial Atlantic on the El Niño Southern Oscillation. *Clim Dyn* **38,** 1965–1972 (2012). https://doi.org/10.1007/s00382-011-1097-y

Hamouda, M. E., and F. Kucharski, 2019: Ekman pumping mecha- nism driving precipitation anomalies in response to equatorial heating. Climate Dyn., 52, 697–711, https://doi.org/10.1007/s00382-018-4169-4.

Kucharski F, Bracco A, Yoo JH, Tompkins A, Feudale L, Ruti P, Dell’Aquila A (2009) A Gill-Matsun-type mechanism explains the Tropical Atlantic influence on African and Indian Monsoon rainfall. Quart J R Met Soc 135:569–579

Kucharski F, Bracco A, Yoo JH, Molteni F (2007) Low-Frequency variability of the Indian monsoon–ENSO relationship and the tropical Atlantic: the ‘‘weakening’’ of the 1980s and 1990s. J Clim 20:4255–4266

Kucharski F, Bracco A, Yoo JH, Molteni F (2008) Atlantic forced component of the Indian monsoon interannual variability. Geophys Res Lett 35. doi:10.1029/2007GL033037

Losada T, Rodríguez-Fonseca B, Polo I, Janicot S, Gervois S, Chau- vin F, Ruti P (2010) Tropical response to the Atlantic equatorial mode: AGCM multimodel approach. Clim Dyn 35(1):45–52. https ://doi.org/10.1007/s0038 2-009-0624-6

Rodríguez-Fonseca, B., Polo, I., García-Serrano, J., Losada, T., Mohino, E., Mechoso, C. R., and Kucharski, F. (2009), Are Atlantic Niños enhancing Pacific ENSO events in recent decades? *Geophys. Res. Lett.*, 36, L20705, doi:10.1029/2009GL040048.

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

We propose to rewrite this point.

"Considering the tropical basins separately, an anomalous warming of the eastern equatorial Atlantic induces a dipolar rainfall response over West-Africa in boreal summer: a decrease of the rainfall in the Sahel region and an increase of the rainfall over Guinea Coast. However, below normal sea surface temperatures in the eastern tropical Pacific lead to an increase of the rainfall in the Sahel. After 1970s, the coupling between the eastern equatorial Atlantic and the eastern tropical Pacific has strengthened, and the two basins are characterized by an opposite phase relationship. Therefore, a positive phase of the Atlantic Niño is associated with negative SST anomalies in the eastern tropical Pacific. This leads to rainfall anomalies of opposite signs over the Sahel, which damps the West-African dipolar rainfall response associated with the Atlantic Niño (Losada et al 2012)"

Losada T, Rodriguez-Fonseca B, Mohino E, Bader J, Janicot S, Mechoso CR (2012) Tropical SST and Sahel rainfall: a non-stationary relationship. Geophys Res Lett. https ://doi. org/10.1029/2012g l0524 23

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.

This discussion is removed as suggested.

"Results from the General Circulation Models (GCMs) participating in the Coupled Model Intercomparison Project (CMIP) show that from the fifth phase (CMIP5) to the sixth phase (CMIP6) of the Coupled Model Intercomparison Project, the surface temperature biases have been reduced over the tropical Atlantic, as pointed out by Richter and Tokinaga (2020) in an analysis of the pre-industrial control experiment performed with 33 models."

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

CMIP6 historical simulations are forced with observed natural and anthropogenic forcings. We will modify the text accordingly.

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

Yes, we mean SSP5-85, we will modify the text accordingly.

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.

We will keep ERA5 reanalysis for comparison with the model outputs as our main conclusions do not depend on the choice of the selected observation product. The other observed rainfall and SST datasets will be removed.

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.

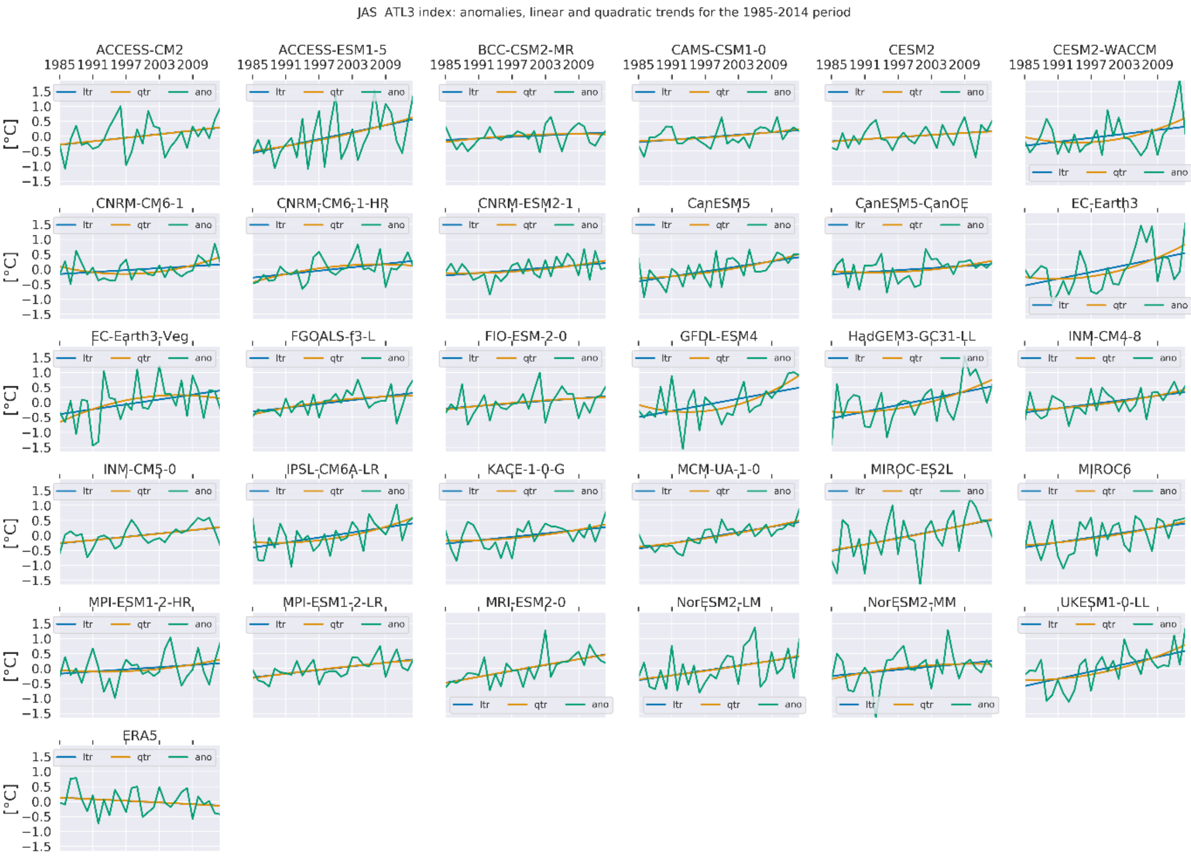

Figure R 1. SST indices of the Atlantic Niño: JAS mean of monthly SST anomalies averaged over the Atlantic Niño area, for the 1985-2014 period (green curves). The linear (blue curves) and quadratic (orange curves) trends are superimposed on each panel. SST outputs from CMIP6 historical simulations (30 GCMs) and the ERA5 reanalysis are considered.

From Fig. R1, we noted that the quadratic trend does not differ from the linear in much of the models (e.g., ACCESS-CM2, MRI-ESM2-0, NORESM2-LM). However, there are some cases where both trends behave differently, e.g. EC-Earth3-Veg, GFDL-ESM4. This motivated us to consider the quadratic trend which, would better follow the changes in the trends inside each time series.

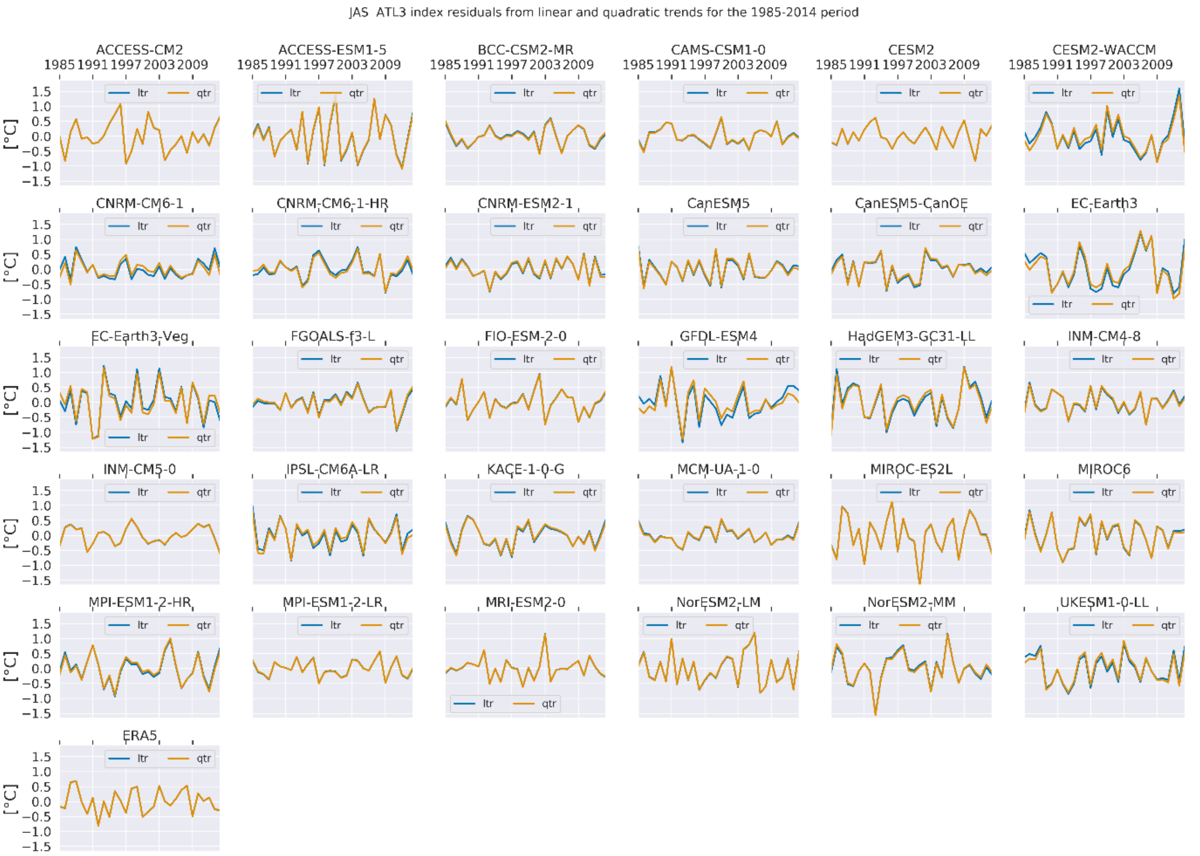

Figure R 2 Residuals of the detrended JAS ATL3 index after removing the linear trend (blue curves) and the quadratic trend (orange curves). The displayed 1985-2014 time series are from 30 CMIP6 models and ERA5.

The residuals from the linearly detrended SST time series are considered in Fig. R2. Results show that there is no substantial difference between the residuals when the linear or the quadratic trends are removed. Therefore, for simplicity, we will consider detrending linearly the different datasets in the revised manuscript.

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. Atl3 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 Atl4, tropical North Atlantic (TNA) and tropical South Atlantic (TSA)?

We will remove the acronym ATL3B, and keep ATL3 describing the box related to the Atlantic Niño center of action.

TAB1 and TAB2 are two domains used in our work to validate the SST and rainfall patterns, respectively, related to the Atlantic Niño mode. In this validation process, we wanted to consider a large spatial domain that takes into account both the northern and southern parts of the tropical Atlantic, which is not the case for the TNA and TSA regions. The ATL4 region considers only the western part of the equatorial Atlantic, which is too narrow relative to our objective.

We will add more argument in the description of the domains in the revised version of the manuscript.

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

We would like to thank you for this suggestion. This section will be reorganized like this:

2. Data and methods

    2.1. CMIP6 data
    2.2. Reanalysis
    2.3. Analysis strategy

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.

The question of annual cycles and seasonality will be moved to the supplementary material, and we will focus the discussion on the seasonal biases and patterns as suggested.

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).

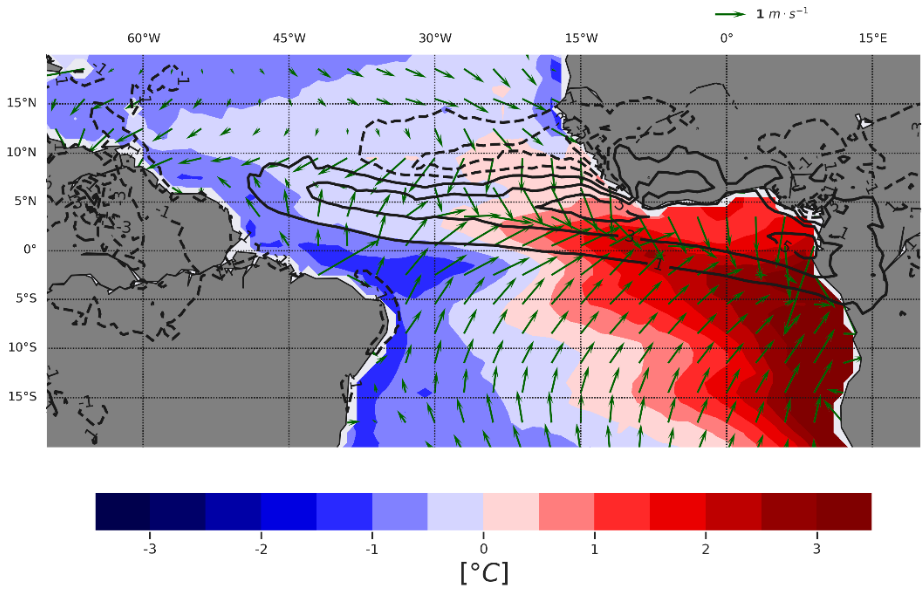

Figure R 3 Ensemble mean of the JAS SST (in colors), rainfall (in contours) and 10 wind (arrows) biases relative to ERA5 over 1985-2014. Theses biases are computed from 23 GCMs for which this field is available.

The strong easterlies shown in our figure (north of 10°S in the eastern basin) are part of the anomalous northerly flow that brings the moist air into Guinea Coast, favoring a positive rainfall bias. This inflow is strong at 850 hPa (roughly 1.5 km above the sea level and this is the reason we selected this level on Figure 2.). The pattern is different from the one of the bias in the near-surface flow (10 m) that is shown in Richter and Tokinaga 2020.

In Figure R3, we show the anomalous near-surface westerly biases for JAS, which is consistent with Richter and Tokinaga 2020. This figure will be added to the supplementary material and briefly discussed.

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.

We will refer directly to the variables (rainfall, SST, etc), by removing the regression coefficient terms.

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.

We have decided to keep only the ERA5 reanalysis to compare with the CMIP6 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 both pattern correlations and variance (that is closeness to REF).

This figure will be split into two, as suggested.

The standard deviation of the SST spatial patterns related to the Atlantic Niño in the different models is not scaled by its corresponding value in ERA5. This is why the standard deviation of the reference is different from 1 in our Figure (4b). Scaling the data will not change the figure, and we chose to have the estimation of the deviation from the spatial mean of the pattern. We will add this additional information to the title of the figure. Overall, the models show a good representation of the SST spatial distribution associated with the Atlantic Niño, with an overestimation of the SST amplitude.

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.

We will split the figure into two, as suggested, and we will consider ERA5 reanalysis for the model evaluations.

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-→SST(atl3) 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?

As suggested, the reasoning below the analyses will be explained in the revised version. During positive phases of the Atlantic Niño, warmer than normal sea surface in the eastern equatorial Atlantic weakens the zonal surface pressure gradient, which in turn weakens the prevailing trade winds. The regression of the low-level zonal component of the wind onto the ATL3 index is used to evaluate the first component of the Bjerknes feedback, which is the forcing of the surface wind in the west basin of the Atlantic Ocean by SST in the eastern basin. Then, these anomalous westerlies increase the surface convergence above the warm waters in the east, which leads to a rising of the sea surface height, an increased heat content and a deepening of the thermocline. This is the second component of the Bjerknes feedback. Then, the deepening of the thermocline reduces the influence of the upwelling of cold subsurface water on the surface temperature, which then reinforces the initial surface warming. This is the third component of the Bjerknes feedback, which we accessed by regressing the sea surface height, a proxy for the thermocline depth, onto the ATL3 index.

Are the rainfall and divergence related to the ITCZ/atmospheric component of the Bjerknes feedback (Nnamchi et al. 2021)?

Yes, the rainfall and divergence are related to the atmospheric ITCZ component of the Bjerknes feedback. This is related to the spurious southward position of the mean ITCZ position in the climate models relative to the observations during the boreal summer. This bias would lead to an enhancement of the coupling between the atmosphere and the ocean, during the growing phase of the Atlantic Niño in the models. We will add a short discussion of this point inthe revised manuscript.

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

The percentage of change of the ATL3 index standard deviation between two periods is computed as $100 \times \frac{\sigma_{fut}-\sigma_{his}}{\sigma_{his}}$, where σ_{his} is the standard deviation of the JAS ATL3 index in the 1985-2014 period, and σ_{fut} the standard deviation of the JAS ATL3 index in a future period (the near-term, mid-term or long-term periods). This information will be added to the revised version of the article.

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?

There could be several reasons to explain the differences between our results and those from Brierley and Wainer (2018). Among others, we can postulate on the differences in the models between the CMIP5 and CMIP6. Brierley and Wainer (2018) compared a 1% per year quadrupled CO2 experiment to a pre-industrial control simulation of CMIP5, which is different from the simulations compared in our analysis (historical and SSP5-8.5 simulations). A better comparison between the two studies could be performed by analyzing the ATL3 variability changes between CMIP6 1pctCO2 and the CMIP6 pre-industrial simulations. We will add this comment to the revised manuscript.

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.

We will recall the three elements of the Bjerknes feedback at this point.

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.

We will rewrite this point, by replacing the tropical Atlantic rainfall teleconnection by the equatorial Atlantic rainfall variability. Thank you for this correction.

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

We will remove the discussion about the Saharan Low and keep only the link between the mean state change along the equatorial Atlantic and the change of the Atlantic Niño variability.

References

Brierley, C. and Wainer, I.: Inter-annual variability in the tropical Atlantic from the Last Glacial Maximum into future climate projections simulated by CMIP5/PMIP3, Climate of the Past, 14, 1377–1390, https://doi.org/10.5194/cp-14-1377-2018, 2018.

Jouanno, J., Hernandez, O., and Sanchez-Gomez, E.: Equatorial Atlantic interannual variability and its relation to dynamic and thermodynamic

processes, Earth System Dynamics, 8, 1061–1069, https://doi.org/10.5194/esd-8-1061-2017, 2017.

Keenlyside, N. S., & Latif, M. (2007). Understanding Equatorial Atlantic Interannual Variability, Journal of Climate, 20(1), 131-142.
https://journals.ametsoc.org/view/journals/clim/20/1/jcli3992.1.xml

Nnamchi, H.C., Latif, M., Keenlyside, N.S. et al. Diabatic heating governs the seasonality of the Atlantic Niño. Nat Commun **12,** 376 (2021). https://doi.org/10.1038/s41467-020-20452-1

Richter, I., Tokinaga, H. An overview of the performance of CMIP6 models in the tropical Atlantic: mean state, variability, and remote impacts. Clim Dyn **55,** 2579–2601 (2020).
https://doi.org/10.1007/s00382-020-05409-w