**Climate
of the Past**
Discussions

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**Interactive
comment**

Interactive comment on "Interannual Variability in the Tropical Atlantic from the Last Glacial Maximum into Future Climate Projections simulated by CMIP5/PMIP3" *by* Chris Brierley and Ilana Wainer

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article

1   Editor Comments

Dear Author,

The reviews process of your manuscript it taking a long time, which is due to recurrent difficulties to get a second review. I am very sorry about it. Since time is running I propose that you go for a major revision of your manuscript, taking into account the important comments of Referee 1 who raised important issues on the clarity and organization of the paper, including methodological questions. I also include below my own expertise of the manuscript, focusing on major aspects. If you decide to provide a revised version of the manuscript it will be sent for a second round of reviews, with the hope that we do not have to face a long delay similar to the one of the first round.

Best regards

Pascale Braconnot

Thank you for considering the length of time so far needed to find sufficient reviewers. We appreciate the opportunity to revise our manuscript in light of the comments so far from both yourself and Referee 1. We have outline how we would address these comments in the bold in the document below.

1.1   Comments on the manuscript.

The subject is quite ambitious and timely, and the methodology used to discuss the two major Atlantic modes seems appropriate. As far as I know this has not been done yet, and providing systematic diagnoses to assess how the modes of variability are affected by climate change is a valuable task. In its present form however the paper is too descriptive and key aspects on precipitation are lacking. In particular :

- The introduction and first section highlight the fact that AMM and Atl3 modes have fundamental impact on South American and African monsoon, but this linkage is not discussed any further when considering the different climates. This limits the interest of the manuscript and is a major concern.

We shall revise the text with this insight in mind. The later comment about the variability explained would provide a sensible approach to incorporate the monsoon in the discussion.

- The discussion on physical and dynamical mechanisms should be enlarged. This concerns both the anomalous circulations associate with the SST modes and the changes in these circulations associated with changes in mode patterns in the different climates

We shall enhance this style of discussion in a revised version.

- One of the difficulties with the analyses of paleoclimate simulations is that both the background climate mean state and the variability change. How the pattern of the changes in variability is connected with patterns of the changes in the mean state should be discussed in more depth. A question out of this is does mode patterns only follow the mean state patterns? In other words if there is shift in the mode pattern is it directly reflecting a shift in the mean state pattern or is there other feedback that could explain that new areas become affected by the mode?

We had attempted to answer these questions with the discussion section and Figs 12 & 13. Clearly these are insufficient (both in light of your comment and the lack of relationships demonstrated within them). We shall incorporate more individual analysis in our revised manuscript.

- The outline of the paper is also a little bit "boring". This feeling is due to the fact that the discussion section could include additional analyses to explain when possible part of the rationale behind model responses (which may be different from one period to the other). The discussion section could thus be enlarged and have a more appealing title and content. It could compare relationships as it is done as well as mechanisms.

We were already worried that the manuscript was too long. But we certainly agree that it is largely descriptive at present, and that further discussion of the mechanisms would increase its interest to readers (and reviewers). We shall endeavour to find some more insights to discuss in the revised manuscript.

A few questions when reading the manuscript:

- It is interesting to see that the AMM mode is reduced at mid Holocene. Is it because the seasonal cycle is stronger and that a dipole-like pattern emerges in summer when comparing mid-Holocene with PI?

That was our suspicion, but we shall investigate further for the revised manuscript.

- Is there a reason why a colder climate would have increased variability?

Recent work by Rehfeld et al (Nature, 2018) propose this would arise from increased temperature gradients. We shall refer to this and others in future.

- Could the non-symmetrical differences between LGM and future results from non-symmetrical responses in mean change in Hadley & Walker circulations between these two climates (related for the Hadley circulation to a dynamical or cooling effect induced by the ice-sheet)?

We are not sure why the non-symmetrical pattern occurs - clearly not because of the SST gradient changes proposed by previous authors as we investigated. The suggestion of changes in the Hadley and Walker cells is an interesting one, which we will look into.

C4

1.2   Other comments

• Please, provide error bars on the different bar plots

We shall investigate whether there is a sensible method to compute the error bars. For some models they can be computed from the preindustrial, but not all.

• Table 1 mentions past1000 simulations, but they are not used in the text.

We have computed the AMM and ATL3 modes from these last millennium simulations (should someone want them). However, you are correct that they do not feature in the manuscript and should have been removed from the table.

• Make sure the color scales are identical for all the plots with the differences. Some of the values are so small that they should not be plotted. Would there be an interest to also show separately the results for models for which the difference is an increase in the index and the models for which it is a decrease? which would require that statistical significance is defined to tell for which models it is different from 0.

We honestly are not sure if there is a value in showing the individual model results in the manuscript. We had intended to provide all the model fields along with some code to visualise them as supplementary information. Nonetheless, we shall investigate these figures.

• For the maps of differences you could add isolines showing the pattern for PI to better highlight where the changes are located compared to the reference.

That is a good suggestion. We will have a go at doing this and see if it enhances the readability of the figures.

- Section 2 should be more informative. Details would be welcome to make sure we understand well how exactly the anomalies are computed for each of the periods, how the regressions are computed to provide the ensemble mean map, and also for each model what is the level of significance for the regression and should non-significant values excluded (or set to 0) when computing the ensemble mean map?

We shall explain our methods in more detail in a revised manuscript, as our current description is clearly insufficient. No significance testing was performed for the regression analysis - we shall investigate the practicalities of undertaking this during the revision stage.

- The estimates of the changes in variability are done using the ensemble mean value. Since the sampling is limited given the size of the model ensemble would it make any difference to consider the median valued?

- In section 3 tell why the observations look so noisy in figure 2.

That is a good question, and one that we were not sure about. We suspect they may be spectral echoes from the Reanalysis model, but shall investigate further.

- In section 3.1 PIcontrol should also be considered with historical to show the differences between this two close periods and discuss the limited length of the simulations.

We were worried that we were already showing too many panels and figures. However as both the Editor and Referee felt this was an important omission, we shall correct it.

- Some of the Pi Control experiments are long enough to be subsampled for an uncertainty analysis.

That is a good point. We will think about how to use them, and if they would be helpful (esp. given Referee 1's point about consistency between the models)

- Make sure the modes are discussed in the same order in all sections and figures.

This structure had not been rigidly observed in an attempt to keep a narrative flow. As it obviously did not help, we will revert to something more strict in future

- Even though the modes are extracted using an index and not EOF you could compute and provide the percentage of variance they represent. Previous studies Jolly et al. 2007 or Zhao et al. 2008 suggested that ENSO dominate variability in most models and thereby the teleconnection with the African monsoon, which is not the case in the observations. Is it valid here?

This is a really helpful suggestion of how to tie the SST variability back to the South American and African monsoons. We shall undertake this analysis in the revised manuscript.

- P12 l230. The sentence is incorrect. Pausata et al. 2017 didn't simulate vegetation better they impose a mid-Holocene extreme reconstruction of the vegetation cover. So it should read something like when imposing mid Holocene vegetation reconstruction as boundary condition to the model.

This was sloppy language on our behalf and we will edit as you suggest.

2   Referee 1

This work investigated the change in the leading modes of the Tropical Atlantic Variability, the Atlantic Meridional Mode (AMM) and the Atlantic Niño (ATL3), in different

climate scenarios: the historical, the last glacial maximum, the mid-holocene and future simulations in the multi-model ensemble of the PMIP3/CMIP5. Authors used this set of experiments in order to find robust signal of change in the Tropical Atlantic Variability. They found that all models across all experiments are able to represent main characteristics of dominant modes of variability in the Tropical Atlantic in spite of the mean state bias.

The paper addressed a relevant question: how the Tropical Atlantic Variability change under different climates and how the information from mean state and past climates can be used as a constraint for the future. They quantified first the mean state model bias of the tropical temperature and precipitation in the historical simulations against reanalyses. After that, they compared the magnitude of the simulated change of the tropical temperature and precipitation in the mid-holocene, the lgm and the future climate with the mean state bias, concluding that the simulated changes are reasonably represented in these experiments. Hence, the main conclusion is that ATL3 and AMM are well represented among models and experiments considered, although authors found weak correlation with change in temperature gradients, so it is not possible to identify emerging constraints for future projections from this analysis.

- I think this is a good work but sometimes the storyline is hard to follow: I suggest to work more on the structure of the paper and on discussion and conclusions in order to clarify main findings (perhaps merging both sections would be helpful).

This suggestion is echoed by the editor. We shall take this onboard during our revisions.

- About the method, I think that conclusions hold only if same models among experiments are considered, otherwise results might be affected by different model physics and also by the different number of the model used for each experiment (see my comments below).

C8

We feel that this is overly conservative, but propose to test the suggestion in a revised manuscript.

- Furthermore, several typos in the captions (see specific comments below) made the paper difficult to read.

- Some recent and important literature is also missing (see the list below).

In a revised manuscript we shall engage better with the literature about the ITCZ, especially in its formal sense.

2.1   Minor Comments

There are some typos in the text. Here, it is a list:

- Ln 39: "It is associated with a shift ...". It is not clear what is the subject of the sentence.

- Ln 63: a full stop is missing after the brackets "(c.f. the AMM)".

- Figure 1: the caption mentioned HadISST for panels a and b, but 20C_reanalysis is written on the top of both panels.

- Figure 2: replace "in precipitation" with "of the precipitation" in the caption.

- Ln 203: You specified already the acronym TAV for Tropical Atlantic Variability in the very beginning of the paper. You can use it throughout the paper.

- Figure 3: Typos in the caption. ATL3 is shown in panel a and c. AMM in panel b and d. In the upper panels observation results and in the lower panels simulation results are shown. Also the standard deviations must be reorder: 0.17 refers to panel a, 0.18 to panel b, 0.05 to panel b and 0.04 to panel d.

- Figure 6, 8, 10: It is better to change the color of dark gray bars in light gray bars. You could also replace the letters associated to each model, with a number or a short name.

- Ln 280: repetition: "... is expected to be still be..."

We apologise that so many typographical errors passed through our proofreading. We shall correct all of them in a revised version.

3 Major Comments

- Ln 36: "...strength of the ITCZ". What do you mean for strength of the ITCZ. Usually, the ITCZ is the latitude of the wind convergence, and or, the latitude of the maximum of the precipitation. Please clarify.

- Ln 40-42: Add literature about the ITCZ. (e.g. Schneider et al., 2014, Bischoff, T., & Schneider, T. , 2016, Green, B., & Marshall, J., 2017).

- Ln 63: cite Schneider et al., 2014.

- Ln 68: You might want to cite also D'Agostino et al., 2017. They linked Hadley Circulation changes also to change of the meridional temperature gradient and interhemispheric thermal contrast.

- Ln 166-168: What do you mean for: "models are unable to get the full intensity of the ITCZ"? I would like to clarify again that the ITCZ is a latitude of the maximum of the tropical precipitation not the rainfall intensity itself. Please reword, otherwise add specific analysis on the ITCZ shift, including also how to define the ITCZ in the methods.

- Ln 335-340: these statements about the ITCZ are not supported by the analysis shown in the paper. How did you quantify the ITCZ shift? Please include further analysis

We confess that we had used ITCZ as shorthand for rain within the zone, rather than its formal definition of as a location. So, for example, by the text in lines 166-168 we mean there was insufficient rain falling within the ITCZ. We apologise for this sloppiness and will correct in a revised version. We had not undertaken a formal analysis of the ITCZ location. We shall look into the logistics of performing such an analysis for the revised manuscript.

- Ln 225, the Mid-Holocene section: the Tropical Atlantic is elsewhere cooler than Pre-Industrial and this is pretty consistent among models. I was wondering if you believe to this result if the magnitude of the change is weaker than the change in the mean state.

We suspect this result is robust, but have not yet thought of a convincing mechanism to explain it. We shall comment on this in the revised manuscript.

- Figure 4: I have a question about these panels. How did you perform the difference between the multimodel ensemble mean for each experiment and the multimodel ensemble mean of the Pre-industrial condition? You must use different pre-industrial multimodel ensemble mean for each experiments, because you must account for different model list. I did not find any specification about it in the paper.

We apologise for the lack of precise explanation. We have averaged the *changes* from each model - as we thought was standard approach. We shall revise the methodology to be much more explicit about this

- Furthermore, I have some doubt: I don't think the mean state bias can be used to give credibility to results of the different experiments in this context, because the ensemble mean of the historical experiment accounts for 14 models. The ensemble mean of the pre-industrial for 21. They tell different story then. The difference is much more evident if you compute different pre-industrial ensemble mean for each experiment! Therefore, when you compare the climate change with the mean state bias, you are wrong because the different model list. I suggest to restrict the model list to common models only for all experiments. Unfortunately, the list is very short (only 9 models) but I think that is still possible to reach robust conclusions.

We understand your point. We had assumed the biases to not vary much between the historical and preindustrial experiments and the various ensembles. We shall repeat the analysis with the subset of model consistent across all experiments to check this assumption for the revised manuscript.

- Ln 235: add also dust and cite Erger et al., 2016.

- Ln 316: please quantify a "little relationship".

- Ln 330: please quantify again "little robust relationship".

By 'little', we meant neither visually nor statistically significant. We shall rephrase We understand your point. We had assumed the biases to vary much between the historical and preindustrial experiments and the various ensembles. It is easy enough to repeat the all the analysis with the subset of model consistent across all experiments to check this assumption.