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Climate of the Past Discussions
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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

Anonymous Referee #1

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This work investigated the change in the leading modes of the Tropical Atlantic Variability, the Atlantic Meridional Mode (AMM) and the Atlantico 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.

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Discussion paper

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 respresented 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).

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).

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).

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. . .”

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 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.

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. 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 contest, 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.

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

Ln 316: please quantify a "little relationship".

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

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.

Suggested literature:

Bischoff, T., & Schneider, T. (2016). The equatorial energy balance, ITCZ position, and double-ITCZ bifurcations. Journal of Climate..

Green, B., & Marshall, J. (2017). Coupling of Trade Winds with Ocean Circulation Damps ITCZ Shifts. Journal of Climate.

D'Agostino, R., P. Lionello, O. Adam, and T. Schneider (2017), Factors controlling Hadley circulation changes from the Last Glacial Maximum to the end of the 21st century. Geophys. Res. Lett.

Egerer, S., Claussen, M., Reick, C., & Stanelle, T. (2016). The link between marine sediment records and changes in Holocene Saharan landscape: Simulating the dust cycle. Climate of the Past.

Schneider, T., Bischoff, T., & Haug, G. H. (2014). Migrations and dynamics of the intertropical convergence zone. Nature.

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