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Interactive comment on "Inter-hemispheric seasonal comparison of Polar Amplification using radiative forcing of quadrupling CO_2 experiment" *by* Fernanda Casagrande et al.

Anonymous Referee #2

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The authors studied the seasonality of the Polar Amplification (PA), here defined as the difference between the CMIP5 control (piControl) and the abrupt increased CO2 (abrupt4xCO2) experiments. The manuscript shows that the Arctic is more sensible to the PA, which is more remarkable in autumn and winter. I think this is a timely and interesting topic that certainly deserves attention from the scientific community. Also, the paper fits well in the short-communication format (I am assuming this is the case). In my opinion, the manuscript has potential, but the analyses presented so far are too shallow and it should be further improved before publication. First, I think the authors should use all the CMIP models available, and not only 6 of them: CMIP5 has 31 models that performed the abrupt4xCO2 experiment. If there is a good reason to

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use only these 6 models, this should be acknowledged in the text. By using a larger number of models, the authors would be able to perform some statistical analysis (e.g., to compare BESM against the others) and bring robustness to the manuscript. Second, it would be much more useful to the scientific community to see this study conducted with the CMIP6 outputs. Again, the data processing and analyses performed so far are straightforward so that it should not be a problem to adapt them to the CMIP6 models. Third, although I understand that is fair to use the abrupt 4xCO2 experiments in this study, the authors could bring other experiments to their analyses such as the 1pctCO2. Fourth, as the first reviewer also pointed out, I also think that many of the hypotheses raised by the authors could be effectively tested with the CMIP data. Fifth, I miss in the introduction a strong point on what this manuscript brings as new results and, I also missed a more comprehensive conclusion for the new findings. Finally, a bit more care with manuscript writing is required. I have pointed out some mistakes below (not exhaustively), as well as other comments that could be considered by the authors for improving their manuscript.

Other comments

Pg. 1; L. 8: “The numerical climate simulation from Brazilian Earth System Model (BESM) are...” – Replace “are” by “is” or “simulation” by “simulations”.

Pg. 1; Ls. 18, 19, 21, 24: Consider to add an article in the following cases – “warming at the surface”, “heat in the atmosphere.”, “for the cold season”, and “in the coming decades”. Also, for other instances in the manuscript.

Overall comment: For uncountable nouns, the use of the indefinite article “a” may be redundant. For instance: “a warming”, “a cooling”. This rule could be considered for the entire manuscript.

Pg. 2; Ls. 31: I guess the authors meant GHG rather “GHC”.

Pg. 2; Ls. 35–39: The sentence is confusing. It is kind of hard to get what the authors

mean. Please, consider to rewrite it. For instance, “these two-poles inter-hemispheric asymmetries in the mean ocean circulation” but nothing was mentioned for the “Arctic mean circulation”.

Pg. 2; L. 37: “According Marshall...” replace by “According to Marshall”. Please, check for the other instances in the text.

Pg. 2; Ls. 40–42: “Numerous...” but only Vaughan was cited.

Pg. 2; Ls. 45–46: “from between 1875 and 2008” – Drop “from”.

Pg. 2; Ls. 46–47: Add “the” in “latitudes of the northern hemisphere”.

Pg. 2; L. 55: Replace “this processes” by “these processes”; Also, it seems that the explanation “Ocean is becoming more like the Atlantic ocean” is not required.

Pg. 2; L. 59: “The large differences among the models is” – Replace “is” by “are”.

Pg. 3; Ls. 78–81: I was wondering why comparing the BESM results against only 5 other models rather than the entire ensemble of models? Also, since we are already in the CMIP6, why not make this study with experiments from this phase. In addition, since the 4xCO2 seems a bit unrealistic, I think the use of the simulations forced by “1% per year CO2 increase (1pctCO2; Eyring et al., 2016)” would strength the manuscript.

Pg. 3; L. 81: “The paper was is organized”.

Pg. 3; L. 86: Missing “.” at the end of the sentence.

Pg. 3; L. 93: “an a instantaneous”; “the 21st”.

There is a mistake with numbering sections as per Sec. 3.

Pg. 5; L. 129: “accesses”. Do you mean “assess”?

Pg. 5; L. 128–129: It does not seem to be the case since the discussion for Arctic and Antarctic is, in some instances, merged in Sec. 3.

Pg. 5; L. 135: Replace “assesses” by “assess”.

Pg. 6; L. 138: Replace “This procedure been largely” by “This procedure has been”. Also, the authors argued “largely” but cited only 2 references.

Pg. 6; L. 138: “Contrasting, the tropical warming for both, northern and southern hemisphere, is pretty similar with not so accentuated SAT increase in summer and for regions close to 30N.” – Not sure I agree with this statement. From Fig. 1, it is noticeable an increase in the SAT differences from about -60S to +60N. Could the authors add some words/explanation for that in the manuscript?

Pg. 6; L. 146–147: “. . . the overall weaker warming in Antarctica is due to a more efficient ocean heat uptake in the southern ocean”. I am wondering whether the authors could test this by looking at the SST data (or another output variable). For instance, is the Polar Amplification and respective seasonal cycle also observed in the SST data. If so, what are the differences between Antarctic and Arctic? Maybe something could be shown in terms of albedo feedback. I think this is a better way to address the issue rather than “We expect...”.

Pg. 6; L. 155: “reaching a minimum at 70S” – I would rather say 60S.

Pg. 6; L. 160: “The main reason for winter (DJF) Arctic Amplification pointed by Serreze et al., (2009) is largely driven by changes in sea ice, allowing for intense heat transfers from the ocean to the atmosphere...”. I also think the authors could check this hypothesis with the CMIP datasets.

Pg. 6; L. 163: Replace “looses” by “loses”.

Pg. 7; L. 171: Replace “consequent” by “consequently”.

Pg. 7; L. 174–178: The referred teleconnection seems to be out of context here.

Pg. 7; L. 180: Replace “trend” by “tends”(?)

Pg. 7; L. 190: Replace “In the other hand” by “On the other hand”.

Pg. 7; L. 197: Replace “Artic” by “Arctic”.

Pg. 7; L. 203: Replace “register” by “registered”.

Pg. 8; L. 209: Replace “previously version” by “previous version”.

Pg. 8; L. 208–212: Not sure the comparison between the two BESM versions makes sense in the scope of the manuscript. The paper compares different models but not different versions of the same model. As it is, it seems like an artifact for auto-citation.

Fig. 2 – I think this analysis should be performed for the ensemble of models.

Fig. 3: This figure should be further improved. The labels are too small; it is missing the y-label and unity; the colorbar is not aligned with the figures.

Pg. 11; L. 275: Replace “a combination changes in winds” by “a combination of changes in winds”

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