

Response to Referee 1.

Report #1	
Submitted on 30 Apr 2020	
Anonymous Referee #1	
Anonymous during peer-review: Yes No	
Anonymous in acknowledgements of published article: Yes No	
Recommendation to the editor	
Does the paper contain new data or new ideas or both of them?	Yes No
Are these up to international standards?	Yes No
Is the presentation clear?	Yes No
Does the author reach substantial conclusions?	Yes No
Is the length of the paper adequate?	Yes No
Is the language fluent and precise?	Yes No
Are the title and the abstract pertinent and understandable?	Yes No
Is the size of each figure adequate to the quantity of data it contains?	Yes No
Does the author give proper credit to related work and does he/she indicate clearly his/her own contribution?	Yes No
Would you cite this paper as a scientific contribution?	Very important: Fairly important: May have potential after additional work and resubmission: No potential value

Thank you for your constructive and helpful feedback. We really appreciate the all comments. Appropriate changes were made in the revised/final manuscript according all the suggestions.

Response to Referee 2.

Thank you very much for your consideration. We really appreciate the comments. Appropriate changes were made in the revised/final manuscript according your suggestions (specific comments are below).

Referee #2

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

Even though I find this new version of the manuscript much improved compared with the first version, I have to reiterate my comments from my first revision. I start by saying that, I indeed think that the manuscript addresses a timely and interesting topic that certainly deserves attention from the scientific community. Mainly, I like the "multi-model" comparison of Polar Amplification between the tw hemispheres. In this direction, I congratulate the authors.

However, I feel like the authors didn't address most of my main comments, as follows:

1. In my opinion, the authors are using outdated data sets. We are already in the CMIP6 phase. This means that most of the models have already involved, corrected errors, improved parameterizations, etc, and so generated new data. The CMIP6 data sets are already available, and the computations performed by the authors are not that complex. They could easily be applied to the CMIP6 datasets. I am not saying that the CMIP5 data is not useful, but it would be more useful for the scientific community to see this multi-model comparison with the CMIP6 data. Or, it would be interesting to evaluate what are the differences between the Polar Amplification represented by the models from CMIP5 and CMIP6 phases. To be sure that I am not being impartial, I raised this discussion with other researchers involved in CMIP6 and this is indeed a kind of common sense in the community.

Indeed, we have update the number of CMIP5 models, which now includes 15 models, as well as, we have include 17 CMIP6 models, for both, piControl and Abrupt 4xCO2 protocols.

2. It doesn't help the fact that the authors are using a very limited number of models for a CMIP-like comparison. Since the authors are using relatively old data sets, the minimum that we could expect to see is a broader comparison with all models running the piControl and 4xCO2 experiments. To this point, the authors argued that there is even a Nature Climate Change paper (Harrison et al., 2015) that used only a few CMIP5 models. I don't see this as a valid argument for the following reasons: (i) this paper was published 5 years ago, so that I am not sure how many models had already contributed to CMIP5 by the date when the authors submitted their manuscript; (ii) back then, the CMIP5 was the current phase of CMIP, while now we are in the sixth phase; (iii) one of the objectives of Harrison et al. (2015) was, as I have mentioned above, to evaluate the "improvements in model performance between CMIP3 and CMIP5 in the simulation of large climate changes"

(see their goal n. 4); (v) as far as I understood, Harrison et al. (2015) used data from PMIP, a CMIP-endorsed project so that not all contributors to CMIP5 had run the paleo-simulations. This is not the case for the piControl and 4xCO2 experiments since many of the CMIP5 contributors have provided with those runs.

We agree. As pointed out in the response to the first point, we have now a sum of 32 models, which we believe encompasses the Polar Amplification phenomenon that we are focusing on in this manuscript.

3. I recall that in the first version of the manuscript the authors had based their conclusions on only three figures and, at this stage, it wasn't clear whether the manuscript was a short communication or a full article. So, I have suggested that the authors could also use the 1pctCO2 runs. In my opinion, this could bring robustness and make their study more complete. I am not sure what the authors think about this since they didn't present any answer to this comment, even though I have raised this suggestion both in the main and specific comments. In any case, I am not arguing that these analyses are a "must" for their study, but they would certainly make their manuscript looks like a full-article version (what it is still not clear for me).

Again, we agree with the reviewer's concerns and have restructured the manuscript accordingly. The new manuscript version, which includes more CMIP5 and CMIP6 models, presents a new range of Polar Amplification for both, Arctic and Antarctic. Also, differences in the seasonal values of Polar Amplification.

Based on the fact that I didn't see my first comments properly addressed, I am afraid that I can't give further recommendations on this manuscript's version. I am sorry that I can't be more positive at this stage.