Review for:

The Cryosphere Discussions

Impact of atmospheric forcing uncertainties on Arctic and Antarctic sea ice simulation in CMIP6 OMIP

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https://doi.org/10.5194/tc-2022-110

The manuscript "Impact of atmospheric forcing uncertainties on Arctic and Antarctic sea ice simulation in CMIP6 OMIP" by Lin et al. 2022 presents a study, that addresses the impact of different atmospheric forcing on the CMIP6 Ocean Model Intercomparison Project (OMIP). The paper focuses on the simulated sea ice condition by three OMIP models that provide sea ice tendencies for a complete analysis of the results.

The study is well structured and organized. It addresses the newest model versions of model intercomparison project, the CMIP6. Methods and data are appropriate, and the results are clearly presented and described (text and figures). The referencing is good. The paper presents a useful report of the model experiments with different forcing datasets, but could profit from more ambitious interpretation of the physical processing behind the results, and evaluation of the atmospheric results against reference datasets. Here I suggest three main points that the authors could consider to further improve the manuscript:

- Comparison of the forcing datasets (COREII and JRA55-do)
    - You could compare the surface energy fluxes and the near surface wind speeds of the two input datasets. This would allow to say how directly the differences in the input data are transferred to the model results
- More focus on the evaluation and interpretation of the energy balance terms
    - Are the model results realistic, are OMIP2 results better that OMIP1? You could compare the modeled fluxes against literature values or reference data. I understand that finding a good reference dataset for polar energy fluxes is hard. Still, it would be possible to compare the fluxes against reanalyses (eg. ERA5, JRA55 or NCEP-CFSR) to get an idea of the possible biases. Another approach would be to compare each model against the multimodel mean (mean of CMCC-CM2-SR5, MRI-ESM2-0 and NorESM2-LM), assuming that the multimodel mean is better than each individual model.
- Evaluation of the sea ice thickness
    - There are substantial biases in the sea ice thickness. Can you comment them in any way? Is the reference data reliable (see also my comment for Page 17, lines 399-400), can the limitations in model physics make the ice too thick, what in the forcing data or initial conditions could cause the ice to be too thick?

These improvements could make the paper even more relevant to the audience and turn the "clues on how improved atmospheric reanalysis products influence sea ice simulations" to more than just clues. However, even in the current form, the paper points out the aspects where more research is needed, which is also very relevant for the community. All in all, this a

meticulously prepared manuscript, where the expertise of the authors is visible. Thank you for the interesting reading!

I provide some more detailed comments below.

Page 3, Line 87: In this paragraph, you could summarize some main links between the atmospheric circulation and sea ice conditions. You could describe the role of atmospheric circulation and it's impacts on sea ice on a general level. You could mention that some circulation patters allow heat and moisture transport to the Arctic, while others prohibit it.

Page 5, Lines 165-166: To help the reader, you could mention that SIC was overestimated in CCA in OMIP1 and that overestimation was reduced in OMIP2.

Page 8, Table 1: The caption states: "The improvements on ice concentration simulations and the related reasons in summer (bold) and winter (bold italic) are marked". Bold and italic text is used also for the sea ice concentration tendencies, the surface heat flux, and the surface stress on sea ice. Is this correct, or should the improvements be marked only for the SIC?

Page 13, line 319: You wrote "downward net shortwave radiation flux" do you mean the net shortwave or the downward shorwave?

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Page 16, Table 2: To me, it would be more intuitive to see the OMIP1 (C) and OMIP2 (J) values instead of C and J-C. Best would be to have all three values (C, J, and J-C), but it's hard to fit them in one table. Also, I would suggest adding the downward longwave and upward longwave radiation, as you did for the shortwave radiation. You will see if the decreased downward shortwave radiation coincides with increased downward longwave radiation. This would suggest an increase in atmospheric moisture/cloudiness between the two experiments.

Page 17, line 390: Could you add a definition or equation for the MKE?

Page 17, lines 399-400: Could you select the modeled sea ice thickness for a domain that matches the Envisat data (< 81.5°N)? This would allow a better comparison between the datasets.

Page 17, line 402: Should "ice drift speed" be "ice-motion MKE"?

Page 17, lines 404-405: Should "ice drift speed" be "ice-motion MKE"?

Page 18, Figure 6: The interior-bar in June is all green. I think that the blue and orange bars might be missing/hidden.

Page 19, line 434: "drift speeds" or "MKE"?

Page 22, line 515: “More attention needs to be paid to the radiation fluxes and wind stress in the atmospheric reanalysis products.” Here you could elaborate on what kind of attention is needed. What are the sources of the current problems, what caused the improvements when passing from CORE-II to JRA55-do etc.