Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2022-836-RC1, 2023

Comment on acp-2022-836

Anonymous Referee #1

Referee comment on "Constraints on simulated past Arctic amplification and lapse rate feedback from observations" by Olivia Linke et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-836-RC1, 2023

The authors combine CMIP6 model output with reanalysis data, observations and LES model results to investigate the inter-model spread in Arctic amplification (AA) and the Arctic lapse-rate feedback (ALRF). When sorting models into models with stronger and weaker AA and ALRF, strong AA/LRF models better match reanalysis trends in heat advection, whereas weak AA/ALRF better match observed present-day inversion strength.

The presented data and work is interesting and relevant to important research questions, but I have a few major concerns on how the model-observation analysis is carried out:

- The authors do not investigate the role of internal variability for model results Investigating only one ensemble member per model without regard for the ensemble spread might not do justice to models – even a clear mismatch with observations does not rule out that the model in question is consistent with the observed trend or phenomenon (see eg Notz 2015).
- Important conclusions rely on small subsets of the analysed models, comparing only the top and bottom three models in terms of AA/ALRF. For the weak AA group, these are clear outliers in the CMIP ensemble, and two of the three are different versions of the same model. Would the results remain the same (just with weaker signals) if models 4-8/24-28 were used instead?
- The definition of AA as a difference dT_Arctic -dT_global rather than a ratio dT_Arctic/dT_global is surprising to me. Wouldn't one expect most mechanisms driving AA to act in a multiplicative rather than additive way? Similarly, the choice of the reference period is unclear to me. If no observations from the reference period are used, why not choose an earlier reference period (PI or at least 1850-1880 historical) to maximize the signal?

Minor comments:

ll 22 ff and elsewhere in the manuscript: Now that the work is done, I feel that the manuscript would be stronger by focusing on what has been achieved rather than what the authors want to achieve.

l. 65 ff: The impact of clouds on the vertical temperature profile has not been introduced at this point in the manuscript.

l 205: showing that 2019/2020 is equivalent to 2009-2014 using scenario output would be stronger than just assuming it – strong changes have happened in the Arctic in the early 21st century.

For the comparison with radiosondes, I would recommend coarsegraining the radiosonde profiles to the vertical resolution of the models at least as a sensitivity test (same for NSA).

Section 2.4: Comparing March/April measurements with DJFM model data – did you check that model data looks similar for March as for the entire winter season? Do we expect the 1993 campaign to show the same climate state as the 2019 campaign?

l 385: do all models have similar inversion strengths in the reference period?

l 407: what is the timeframe covered by the Kahl (1990) study? Do we expect it to be representative of 2020 conditions?

l 487: what significance level? How did you do the bootstrap analysis?

Fig. 10 and related analysis: This shows data year-round, is there a relevant seasonal cycle?

l. 564: Cronin and Jansen (2016) would be a good reference here

l. 585-590: I think this is an important result deserving a stronger emphasis in the paper, since entrainment has not received a lot of attention in this context so far.

l. 592 "we compile a sizeable amount of observations" Here and elsewhere in the paper: There is nothing to be said against impressing the reader with the large array of observations you bring to the task in addition to CMIP and LES data, but in my view this works better if you leave being impressed to the reader.

l. 687: I think a crucial point here is that CMIP6/s models generate less warming for a given amount of sea-ice retreat. If this is correct, it should be stated more explicitly.

Cronin, T. W., & Jansen, M. F. (2016). Analytic radiative□advective equilibrium as a model for high□latitude climate. *Geophysical Research Letters*, *43*(1), 449-457.

Notz, D. (2015). How well must climate models agree with observations?. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences*, *373*(2052), 20140164.