The Cryosphere Discussions

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Comment on tc-2021-387

Anonymous Referee #2

Referee comment on "Network connectivity between the winter Arctic Oscillation and summer sea ice in CMIP6 models and observations" by William Gregory et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-387-RC2, 2022

Review: Network connectivity between the winter Arctic Oscillation and summer sea ice in CMIP6 models and observations
Gregory et al. 2022
The Cryosphere

General comments
This paper introduces complex networks as a relatively new method to the field of climate science, and applies it to highlight differences between observations and models in the Arctic oscillation, regional sea ice variability and the links between the two. This new method has some interesting potential, and makes the paper a valuable contribution to the field even before considering the later results and insights. The general subject of the paper, Arctic oscillation and its link to sea ice, is one of considerable importance, and this paper offers an interesting angle on the mismatch between models and observations.

In general the paper is well written, with well constructed figures and presents the context for its findings in terms of previous research well. Overall I think that only a few minor edits would be needed before it would be ready for publication.

Specific comments
While I appreciate the use of complex networks in this paper, I'm still a little unsure of what the advantages and disadvantages are of their use compared to more traditional methods like EOF or maximum covariance analysis, even after skimming through Donges et al. 2015. I think that including at least a sentence or two more on this topic would be beneficial. In an ideal world it would be great to directly compare the results (e.g. those in figure 13) with equivalents from EOF analysis, but I appreciate that this would be a substantial effort and could be outside the scope of this paper. To my knowledge, the results in figure 13 are very similar if the AO index from EOF analysis is regressed on observed SIC and that in the CESM1 large ensemble (https://doi.org/10.1175/JCLI-D-20-0958.1, figure 12a and b - not implying that this should be cited, a similar result is probably presented in more detail in some other paper).

Another interesting comparison with EOF based mechanisms is to look at the AO sea level pressure pattern in observations (https://www.cpc.ncep.noaa.gov/products/precip/CWlink/daily_ao_index/ao.loading.shtml). Is there a clear explanation for why the AO doesn't have a strong negative link to the north Pacific in figure 1, while the AO from EOF analysis is so strongly connected to that region?

Line 128 - Could the authors clarify what they mean when they say "as a de-trended (zero-mean) time series data set"? I'm a little uncertain if a linear trend is removed or if it's just the annual cycle that's removed. If no trend removal is done then I think that could have implications. e.g. Line 311 and 370: "In particular, the correlation across the whole Eurasian–Pacific sector of the Arctic has been more strongly negative since 2000". If there's a positive trend in the AO since 2000 (I'm not sure, but it looks like it), and a negative sea ice trend then that could explain the changing relationship between the AO and sea ice, but it might not be causal (e.g. global warming as a confounding variable).

I think when comparing models to observations it would be useful to have a benchmark that accounts for natural variability. For example, it would be interesting to compute ARI and D for CanESM5 by taking an ensemble member as truth (or ideally looping through ensemble members taken as truth). This would give a better benchmark to compare ARI and D to, and could possibly be included on Figure 11. It could also be possible to do a kind of bootstrap where only a random subset of years is selected from observations, but I'm less sure how that would work. In general I think a bit more of an acknowledgement that some of the differences between networks could be due to sampling of internal variability e.g. Line 235: "MIROC-ES2L model produces the most dissimilar network structure relative to ERA5", Line 264: "suggests that the models show large disagreement on the degree of connectivity".

Towards the end of the paper an explanation of some of the mismatch between models and observations on how the winter AO influences summer sea ice, however the majority of the difference in pattern remains unexplained. I just wanted to comment that I hope the authors (or some other reader of this paper) do pursue this issue further using these complex networks in future work, as the results would be extremely interesting.

Technical corrections
258: minor nitpick - fraction, not percentage

389: I would guess this should be models' instead of model's

Figures 13, 15: Should the covariance have some units when comparing the AO to sea ice concentration? Something like % per standard deviation in AO index? I'm not really sure how to interpret the 10^8, particularly as that doesn't really fit in with what I would think units might be.

Figure 14: I’m not really sure what the legend is saying here.