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.

We would like to thank the reviewer for their kind words and for their time given to review this manuscript. Their comments have been thought-provoking and have certainly helped to improve this manuscript. Please see our responses below.

Specific comments

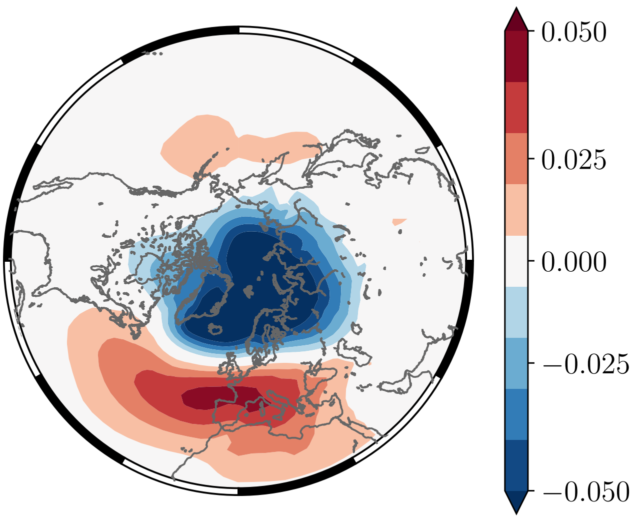
While I appreciate the use of complex networks in this paper, I'm still a little unsure of what the advantages and disadvantages are of their use compared to more traditional methods like EOF or maximum covariance analysis, even after skimming through Donges et al. 2015. I think that including at least 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).
This is a great point, and one which was similarly raised by reviewer 1. We will expand a bit on our answer here: In some specific test cases we would indeed find that the networks approach and EOF analysis produce very similar results. For example, the dominant spatial patterns of variability seen in the network strength map of one particular model ensemble member are likely to also be present in the first few leading EOF maps of that same ensemble member. The purpose of using the networks method in this paper was two-fold. On the one hand, we wanted to use this paper to bring attention to the method itself, as complex networks have become increasingly used in climate science over the last decade, although perhaps not so much in cryospheric/polar domains. Furthermore, it provides a nice initial framework which can be modified for deeper analysis of climatological interactions (in some ways this relates to your final comment below, regarding future work). For example, in this work we have shown somewhat simple network constructions, with some examples of intra-layer connections (i.e., SIC to SIC, or SLP to SLP), and a single inter-layer connection (AO to SIC). Through a bit of graphical manipulation, the same complex networks methodology we use here can be exploited to provide a very visually intuitive understanding of climate interactions with multiple intra- and inter-layer connections across any number of network layers (e.g., Figure 6 in Donges et al., 2015); a feature which would perhaps be quite difficult to achieve with EOF analysis. What's more, the definition of the link weights is very flexible, for example we could in theory apply the principles of causal inference to our networks to prune certain connections, while also accounting for additional factors such as non-linearity. While non-linear forms of EOF analysis do exist, establishing spatio-temporal causality from EOF modes, to our knowledge, is not straightforward.

Some more technical advantages of networks over EOF analysis for example, are that a) EOF analysis is variance greedy, so each of the (EOF) derived modes reflect the direction along which the data exhibit the largest variance, and so patterns of lower variance are inherently masked. b) EOF analysis imposes orthogonality constraints, such that each mode of variability must exist in a direction which is orthogonal to all other modes. In contrast, the networks approach has neither of these constraints. Furthermore, it is also perhaps cumbersome to convey how each of these EOF modes are interrelated, while in the networks framework we can also neatly summarise the interconnected nature between nodes(modes) with the strength and link maps (another motivation for why we chose this methodology). Perhaps an advantage of EOF analysis is that the solution is well defined. Our clustering algorithm here is one of many such algorithms and often what defines a cluster can be quite heuristic. Hence while our algorithm produces a deterministic solution for a network, there are likely many other viable definitions depending on the clustering algorithm chosen. Our algorithm was chosen based on successful previous analysis of ocean teleconnections by Fountalis et al., 2014 (https://doi.org/10.1007/s00382-013-1729-5).

With regards to your final comment above (also thank you for bringing the recent paper by Clancy et al to our attention!), it is perhaps worth mentioning that, in general, the goal of complex networks analysis is to provide a framework in which to construct simple illustrations of complex systems, such as the Earth's climate. While Figure 13 does provide a result which would appear similar to regressing the AO index on gridded SIC, it is in some ways more simple than that. The clustering component of the methodology acts to emphasise the key regions of variability and their spatial extent by dimensionality reduction, so that we can ignore small-scale heterogeneities which might otherwise be related to small-scale processes and/or noise. Indeed, while this is one part of the result, we believe the strength in the methodology also comes from our ability to quantify similarities in spatio-temporal structures by the ARI and D metrics.

In summary, we agree that it is worthwhile including some discussion in the introduction about the similarities/differences between EOF analysis and complex networks, and highlight our reasons for choosing the networks approach. We have incorporated this into the revised manuscript.

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?
The discrepancies are possibly due to different handling of the input data here. The image in the link above appears to be the leading EOF of monthly-mean geopotential height anomalies for the period 1979 – 2000 (for all months of the year), while our analysis was only across winter months, and between 1979 – 2020. In the image below we compute the leading EOF using the same ERA5 data as we use in the manuscript (winter SLP anomalies between 1979 – 2020), where we can see the comparably weaker variability in the Pacific sector compared to the Atlantic.



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).
For all data, the linear trend is removed. We have now stated this explicitly in the revised manuscript to avoid any confusion.

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”.
Thank for you for this great suggestion. A similar point was also raised by reviewer 1, and we agree that some discussion on this needs to be included. To re-iterate our response to reviewer 1: In our case the 20 CanESM5 ensemble members actually comprise two groups of 10 members with different physics (group 1: i1p1f1, group2: i1p2f1), therefore while we can highlight the spread due to internal variability in ARI and D metrics across these members (for each group, physics 1 and physics 2), it will still likely be an under-estimate due to limited sampling. Nonetheless we have included some discussion and additional figures in the revised manuscript.

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.
Thank you for this comment. We agree that further investigation is required to properly investigate this teleconnection, and all the intermediate processes that are affected by the winter AO, that subsequently drive the ultimate response of summer sea ice. We hope that this paper at least lays some initial groundwork for any future investigation, to be continued by either ourselves or others in the community at a later date.

Technical corrections

258: minor nitpick - fraction, not percentage
We have edited this to “fraction”.

389: I would guess this should be models’ instead of model’s
Yes, thank you!

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.
In the revised manuscript we have added units to all plots which show covariance.

Figure 14: I’m not really sure what the legend is saying here.
Apologies, this appears to have been a bug in the file upload. The revised version should contain the correct legend.