Weather and Climate Dynamics Discussions

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# Reply on RC1

Kristian Strommen et al.

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Author comment on "Improved teleconnection between Arctic sea ice and the North Atlantic Oscillation through stochastic process representation" by Kristian Strommen et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2021-61-AC1, 2022

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We thank the reviewer for their constructive and insightful feedback. Before responding to the key points made, we need to point out that last November the authors were able to obtain additional supercomputing units and have since used these to double the ensemble size from 3 to 6. The new ensemble members have proven to be consistent with the original members, which adds considerable confidence to the hypothesis that the stochastic schemes are genuinely improving the teleconnection. All Figures and diagnostics in the revised version of the paper have therefore been expanded to include these members. As a result of this, some details of the discussion have changed. Because most of these changes are in line with suggestions made by the reviewers, we hope this will not cause a nuisance to the reviewers, who might rightfully wish we had waited with submitting in the first place until this larger ensemble was obtained. Unfortunately, it was not clear prior to submission if the required computing units would be obtainable.

- Concerning the choice of sea ice region, we agree that a differing choice for the model and observations leaves us open to accusations of cherry-picking, and at the very least some discussion of sensitivity of results to the choice should have been included. We have now done more extensive testing of the use of different regions and can report the following. If one uses Barents-Kara for all data sets, then the conclusions are qualitatively similar, in that there is a consistent improvement of the ice-NAO correlations when adding stochasticity, and these improvements can be explained using the LIM model. However, quantitatively speaking the results are somewhat weaker, with the correlations in OCE being generally smaller (and not as comparable in magnitude to ERA5) when using Barents-Kara as opposed to Barents-Greenland. We also found that using just the Barents sea for the model gave quantitatively almost identical results to using Barents-Greenland, and the increased ensemble size now singles out the Barents sea anyway (revised Figure 4). On the other hand, the Barents November sea ice in ERA5 has zero correlation with the NAO: it is definitely necessary to extend the region out to the Kara sea for ERA5.

  After careful consideration, we believe it is still justifiable to somewhat adjust the sea ice region in the model compared to observations. The results discussed above have led us to use Barents-Kara for ERA5 and Barents for EC-Earth. The difference between the two regions is therefore even smaller now, with EC-Earth simply omitting the Kara sea. An equivalent table to Table 1 which uses Barents-Kara for all data sets will be included in Supporting Information of the revised paper, and we will clearly highlight and discuss the fact that qualitatively (but not quantitatively) similar results are obtained with this uniform choice. We hope this will go a long way towards addressing the reviewer's objections.

We now expand on our justification. There are two key points. The first is that both the mean state and the seasonal evolution of the sea ice edge is clearly different in CTRL compared to ERA5.It's true that the bias of CTRL and OCE in the mean sea ice in the Kara sea (Figure 1a,b) is on the order of 10% less ice than in ERA5, and this not huge on the face of it. But the biases in the standard deviation (Figure 1c) clearly point to a big change in how far equatorward the ice edge tends to extend to every year: the sign of the pattern (negative near pole, red equatorwards) says that in CTRL, the ice edge tends to extend further outwards. This is important because the heatflux anomalies are dominated by the variations in the location of the ice edge: if the ice edge has moved, so will the largest heatflux anomalies. The 10% difference in the mean state is therefore in all likelihood misleadingly small, smoothing out more important interannual variations in the ice edge in the Kara sea. This change in the seasonal ice edge evolution in EC-Earth3 is further corroborated by the visibly different EOFs (Figure 2). It is true as the reviewer states that the local magnitude of the patterns in the Barents-Kara region are similar between ERA5 and OCE, but clear visible differences still remain. In ERA5, the typical November pattern is evidently an increase (decrease) of ice in Barents-Kara and a decrease (increase) in the Barents sea closer to Russia as well as in the Laptev sea. In OCE, the typical behaviour is an increase/decrease along the entire ice edge from Greenland up to Chukchi. In particular, sea ice anomalies in Barents-Kara may, in the model world, be expected to often come hand-in-hand with sea ice anomalies elsewhere that don't look anything like that of observation. Since it has been noted in previous papers ([1,2] and others that the reviewer themselves provide) that sea ice anomalies in regions other than Barents-Kara may have different, even opposing, impacts on the atmospheric circulation, we do not think such possible effects can be considered negligible.

The second key point is, as discussed in our paper, that there is evidence in the literature that the teleconnection depends on the atmospheric mean state, in particular the position of the storm track. Since the storm track is almost always biased to some degree in climate models, it does not seem unreasonable to suggest that the sea ice region in models best placed to interact with the storm track is slightly different than that in observations.

The fundamental issue here is that external forcing, including that from teleconnections, very often projects onto the dominant modes of variability (e.g. [3,4]). Not only do these differ between models and observations (Figure 2), but in the case considered here, there is non-linearity embedded at both ends: with sea ice as discussed in [1] and with the North Atlantic Oscillation in the visible multimodal behaviour of the jet [5]. We therefore take the view that model biases, in both the mean and the variability, cannot be easily ignored, and indeed many studies have examined the influence of such biases on teleconnections (e.g. [6] for just one recent example). There are also several precedents in the literature for using sea ice EOFs to compute Arctic-NAO teleconnections (e.g. Wang, Ting and Kushner 2017, or the Strong et al paper you pointed us to in your comments), and such approaches would inevitably highlight different regions in models vs observations. It is certainly true that allowing for regions or patterns to shift in models opens up the possibility of cherry picking, and so sensitivity to such shifts should be clearly discussed, which we failed to do. But the flip side is that allowing for no model-dependent diagnostics may overly penalise models and give the impression that model skill (or inter-model consensus) is weaker than it is.

It is the authors' impression that there has perhaps been too little consideration in the literature on potential (small) shifts in the key sea ice region, and we think this is an important point that we wish to highlight as part of our work. The revised version will expand on all the above points to better justify the choice made. Of course, we accept that the reviewer may disagree on some or indeed all of the above points, or be of the opinion that a proper justification of the above points would require more work which would likely be inappropriate to include in this paper. We hope that if this is the case, that our emphasis of the qualitatively similar results obtained with Barents-Kara, and the change from using Barents-Greenland to Barents for the model, will nevertheless allow you to consider your objection adequately addressed.

- We would challenge the assertion that "most models tend to have a weak connection". The range of correlations between Barents-Kara and the NAO found across the coupled CMIP6 models is very well approximated by a normal distribution with mean 0, standard deviation 0.17 and a 95% confidence interval of 0.28. While the exact mean of 0.018 is positive, almost half the CMIP6 models have negative correlations. The EC-Earth3 CTRL ensemble, with its average correlation of -0.06, is in no way an outlier in this distribution and is in fact dead average: this was extremely briefly noted in the submitted paper (line 337), and we have now made this more clear by revising Figure 5 to include the CMIP6 distribution. The inclusion of additional ensemble members has also now produced CTRL members with slightly positive correlations in the period 1980-2015, so there seems to be even less cause to find EC-Earth3 particularly objectionable. Its biases in the mean ice state are also in no way notably worse than many other models.

  Note that the slightly positive mean of the CMIP6 distribution is consistent with findings in earlier literature reviews which report that `most' models show a positive association, but it is clear that this consensus is weak. Another point here is that many of the experiments carried out in the literature are not directly comparable with each other: e.g. many model experiments analysing the role of sea ice use fixed anthropogenic forcings, while the models we consider here are using historical forcings. This may account for any remaining discrepancies.

  That being said, the point that the stochastic schemes may have differing impacts in other models should have been emphasised more. There are examples from earlier work which show consensus across models in some cases and lack of consensus in others. This will be expanded on in the revised manuscript.
- The potential importance of the mean state is a point raised by all of the reviewers, and upon further consideration we agree. It is actually even clearer after having increased the ensemble size that coupling alone isn't sufficient, and it is likely a combination of coupling and mean state. This will be expanded upon further in a response to another reviewer.

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