Weather Clim. Dynam. Discuss., author comment AC2
https://doi.org/10.5194/wcd-2021-61-AC2, 2022

Reply on RC2

Kristian Strommen et al.

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-AC2, 2022

We thank the reviewer for the insightful comments, and bring their attention to the increased ensemble size obtained since submission: see the response to RC1 for further details.

The main concern raised is about Figure 9, which suggested that perhaps OCE was recovering a correct looking teleconnection for the wrong reasons. After doubling the ensemble size the mismatch with observational data has been notably reduced. The improved teleconnection in OCE still appears more driven by the forcing of the ice on the atmosphere, but a clear NAO signal is now also seen for years where the atmosphere drives the ice. We hope this will help reassure the reviewer.

It is perhaps also worth pointing out that we are either way still suggesting that there is "something quite unrealistic" about the CTRL model, to paraphrase the reviewer. We are suggesting that the lack of a teleconnection is unrealistic, and that its improvement in OCE is a genuine improvement. The point being that this is an important result even if CTRL is unrealistic in some ways, because it implies that the considerable intermodel spread in reproducing the observed teleconnection may to a large extent be due to model biases rather than internal variability. If that is the case, then the teleconnection may be much more robust than many studies suggest it is. But in any case, EC-Earth3 does not seem to be a particularly poor model: see the response to RC1 for more on that.

Note that the EC-Earth figures from Blackport et al. 2019 are not reproducible with our data. While the model used is closely related, the EC-Earth experiments considered in Blackport et al essentially use fixed forcings (they use 400 5-year simulations each covering the same period), while our experiments are 65 successive years with historical forcings. Identical diagnostics would not be expected as a result, so we don't see any discrepancies here as a point of concern.

As for plots elucidating the mechanisms more clearly, we produced some additional lag correlation/regression plots between sea ice and heatfluxes (this also being suggested by RC1) as well as some other diagnostics to help clarify. While these do hint at some small improvements in OCE to the daily time-scale local coupling between ice and heatfluxes, our analysis generally suggests that the flaws in CTRL are not clearly visible in the local thermodynamic coupling. Instead, the errors in CTRL appear to be primarily due to errors in the subsequent adjustment and growth of the initial pressure anomaly across the North Atlantic and ice edge more broadly. In fact, this is already what the LIM results suggest,

but this was not really made clear in the submitted manuscript. All this will be discussed (and the relevant new plots included) in the revised paper. Unfortunately, a thorough analysis of errors in the more remote response is not going to be possible to include in this already lengthy paper and will have to be left for future work (though we include some speculation).

Finally, regretfully no time or resources are available to carry out experiments of the sort you describe at present, though we agree they would help. The role of the mean state (also raised by the other reviewers) is discussed in more detail in the revised manuscript in any case, but it has not proven possible to decisively nail down the contribution of mean state vs coupling in our analysis. Besides the complications of local vs remote responses discussed above, it is likely that the inherently non-linear component to ice/heatflux coupling plays a role which our analysis, entirely based on anomalies, cannot detect. Possible non-linear diagnostics that could be explored in follow-up work are discussed in, e.g. Caian et al. *An interannual link between Arctic sea-ice cover and the North Atlantic Oscillation* (2018), Clim Dyn. We hope that the extra diagnostics and discussion, including of potential future work, will satisfy the reviewer anyway.