Weather Clim. Dynam. Discuss., author comment AC3
https://doi.org/10.5194/wcd-2021-61-AC3, 2022

Reply on RC3

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-AC3, 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 details.

- About Figure 6, yes you are absolutely right that there is a 2-way interaction there which we failed to comment on. This will be included in the revisions.

- Yes, there is evidence in prior literature that this teleconnection is weaker in AMIP models. This was mentioned in line 520, citing Blackport and Screen (2021), though we believe earlier studies (cited in their paper) had pointed to this as well. For EC-Earth in particular, the study Caian et al. An interannual link between Arctic sea-ice cover and the North Atlantic Oscillation (2018), Clim Dyn, showed that ice/NAO links are weaker in an AMIP simulation than a coupled simulation, something they attributed to the missing coupling. Our paper provides further evidence to the importance of coupling to get a good teleconnection, though several questions remain about exact mechanisms. We show that while the initial, local ice->heatflux response appears similar for both CTRL and OCE, the subsequent growth and evolution of the anomaly is significantly better in OCE. Presumably, as you point out, the initial local anomaly would be highly realistic in the AMIP simulations, but the failure to propagate the anomaly would likely be even worse given the total lack of coupling. Caian et al. includes some other discussion on possible mechanisms here. We will discuss some simple hypotheses as well, including the alignment of the sea ice edge with the eddy-driven jet, and the importance of sea ice adjustments further afield from the source region (Barents/Barents-Kara). This will be discussed in the revised paper.

- Yes, exactly: the initial anomaly is long-lasting due to the persistence of sea ice, but is ultimately damped away by the opposing response of the NAO. We will revise the paper to make this clearer. More discussion about the initial local response vs more remote adjustments are also included, as per point 2 above.

- All reviewers have commented on the mean state, and in hindsight the minimal role we ascribed to the mean state wasn't justified. We can't see any meaningful difference in

the November 1st initial conditions (of the ice and NAO) between CTRL and OCE, but the LIM model takes anomalies as input, which ignores any non-linear effects. Since such non-linearity is likely to be present here, our analysis can't really address this. On balance, it is likely that the improvements in OCE are due to both the mean state and the coupling, and we will make this clearer in the revised paper.

- The NAO EOF was computed separately for each dataset, to allow the centers of NAO action to shift between each dataset according to differences in the mean state: this will be made clearer in revisions. We believe it is important to allow for some shifts between models to not obscure signals or overly penalise models (i.e. penalising both for mean state biases and changes to modes of variability). That being said, in this case there is little difference between the CTRL and OCE NAO, with a pattern correlation between the two of around 0.97. The results are therefore highly unlikely to change if using the exact same NAO pattern for CTRL and OCE. This will be mentioned in revisions.