Weather Clim. Dynam. Discuss., referee comment RC2
https://doi.org/10.5194/wcd-2021-61-RC2, 2021

Comment on wcd-2021-61

Anonymous Referee #2

Referee 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-RC2, 2021

Strommen and Juricke explore the question of why Arctic-midlatitude teleconnections in climate models are generally weaker than observed by employing a modified version of EC-Earth3 which includes stocasticity in the sea ice and ocean components. The study finds that the Nov. sea ice - winter NAO teleconnection is improved in the model integrations with stocasticity and this appears to be mainly related to stocasticity in the sea ice component. I find this result very interesting; however, like the authors, I am left wondering whether OCE may be getting "the right answer for the wrong reasons". I recommend some major revision of the manuscript to address the key issues below:

1. The authors argue with respect to Figure 9 that OCE and ERA5 have similar daily timescale forcing, suggesting that OCE is getting things right for the right reasons. I'm not entirely convinced of this given that the b coefficient in OCE is larger than ERA5. It would be interesting to see the coupling between ice and other variables using the LIM to provide a bit more evidence that OCE is getting things right, for example the relationship between ice and a variable that is more thermodynamically connected to ice. The authors also note that the difference seen in Figure 9 could be due to chance. If so, can you show similar plots as Figure 9 for each ensemble member of OCE? If chance plays a role maybe there is some evidence of this if all ensemble members are examined individually.

2. Figure 9h and 9i seem to suggest something is quite unrealistic about how this model represents fall sea ice variability. In the Blackport et al. (2019) paper, they examine a version of EC-Earth, EC-EarthV2.3, I believe. Are you able to reproduce their findings with EC-Earth3P used here for the CTRL runs (it would be great to see plots similar to their Fig. 4c, f, and i? It seems that you are getting very different patterns (Fig. 9h), which makes me concerned about the suitability of this model for this study.

3. Could the direct effect of mean state changes be quantified using AMIP-style runs with monthly sea ice and SSTs from the coupled OCE runs? I think it is important to get a better sense of what is going on - is it the stocasticity itself or the effect of the stocasticity

on the mean state. Untangling this has implications in terms of how this study informs model development.

Minor comments:

1. lines 25-30: lots of issues with parentheses that need to be tidied up.

2. line 27: You may want to say "negative NAO" rather than just "NAO" for clarity.

3. Section 2: there are many different abbreviations/acronyms for the model used in this section. After you finish describing the various configurations, can you tell the author which name you are going to stick with throughout the paper? Something like, "Hereafter, the model will be referred to as...".

4. Line 153: What prescribed SSTs and sea ice?

5. line 162: extra parentheses

6. line 218: Figures -> Figure and ssea -> sea

7. Table 1 caption: there is a missing section number - just shows ??

8. line 405-406: I don't think this is the correlation you are showing. It's sea ice and NAO, correct?

9. FIgure 9i does not really look like Figure 9g to me. And it seems a bit strange that Fig. 9h does not look anything at all like Fig. 9g.

10. line 463: FIg. 9 -> Fig. 10