The paper studies winter polynyas in North Greenland. A modeling study using a fully coupled downscaling framework examines the roles of internal variability and long-term sea ice thinning. An examination of the causes and consequences of polynya development on aspects on the energy balance is presented. Model hindcasts are also compared to observations for validation. The key results are that a) the model reproduces key features of three major winter polynyas in the decade of 2010-2020 as well as their meteorological drivers (northward winds>latent heat polynya), b) long term thinning of sea ice did not significantly contribute to their creation, while c) an increase in frequency of wind events that are capable of creating winter polynyas north of Greenland may be responsible for the fact that major events have all occurred within the last decade.

While some of the results are not new, they replicate previous findings (Moore et al. 2018, Ludwig et al. 2019). In particular they use a coupled atmosphere-ice-ocean modelling framework that overcomes some of the limitations of the ice-ocean framework used in Moore et al. 2018 for testing the impact of sea ice thinning. The analysis regarding the increasing frequency of Polynya maker winds seems also new. I think the paper is generally well conceived and executed and makes a significant contribution to the field and I have no requests for major changes. The paper might perhaps benefit from some reorganization that better separates the strongly supported results (lack of role of thinning) from the more "future research is needed" ones (increase in polynya frequency due to wind changes). But that's a matter of taste (and likely personal bias) and I think the paper has lingered in reviewer space for too long already so I don't think that should be a requirement.

Details:

Line 80: Figure 1c. The CFS sea ice thickness looks terrible. Since you are using only above 540 mb probably not an issue for you, but maybe worth a note.

Line 123… "the mean SIC was used to detect … when it dropped below 90%"
I think this needs to be justified since the selection of this threshold probably affects the number of polynyas you would have detected? Does the 90% reflect some kind statistical threshold of variability or a value in the literature? This definition probably has to remain somewhat arbitrary but the sensitivity of results to this selection should be discussed somewhere in the paper.

Line 165 ..SOMs. Seems to me that the SOM analysis is a bit of an overkill in this context and may add more confusion than explanation. I think simple wind (anomaly) composites for the Polynya events would have done the job. I know, you did the work and hey, ML! but maybe save for SOMe other paper?

Line 240 . Looks like Figure 6 is mentioned before Figure 5 (and I don't see a reference to Figure 5). Figure 5 isn't very interesting at this scale anyhow.

Figure 6. Please increase the size. This is hard to see for my aging eyes unless this is improved in production. Also, do we really need to see the full Arctic pattern or is a smaller cut out region sufficient to see what is relevant. What is the significance of the result that this temporal evolution goes through a number of SOM patterns? The key part is northward winds in the region, isn't it?

Fig 7 (S3,S4). Couldn't those be condensed a bit to highlight the years that should be highlighted? What is the grey shading for the standard deviation? I would have expected the standard deviation of the SIC

for that day in the full time series. Why is this larger for the “polynya” cases? See also above comment on defining the SIC threshold. I would have thought a reasonable definition would be 1 or 2 sigma of interannual variability (or quartiles or something like that)?

Line 275: Polynya periods… defined as .

How does this interact with your previous definition of 90% SIC. Needs some clarification.

Line 290.. This section could perhaps get a separate heading “What’s driving changes in polynya Frequency”.

Totally a style thing, but I would lead with testing the hypothesis that is rejected (thinning ice) and corroborates previous results (Moore et al. 2018) and follow with what the more likely hypotheses of “changes in winds”. That would also allow a natural transition to a discussion of the “unanswered” question left for further research (why is the wind changing?).

I like Fig 8 but think that if the increased frequency in wind events is considered a key result, then its statistical robustness may need some additional support.

Line 295 :La Nina winters….

That idea seems to be not sufficiently developed. I think it is ok to document the increase in frequency of polynya making wind events (with some stats) and leave the global context for future research. Unless the idea can be developed better and/or supported through some results in the literature (Tropical/Arctic connections are a whole study area), I’d leave it out.

Line 345… but none as large as 2018

This is left a bit dangling. What does this mean? Why do you think that is? Increase in strong wind frequency or statistical fluke… or you can’t tell at this point?

Comments on prior review.

For independence, I didn’t read the prior review in advance but since the D in TCD stands for “Discussion”, I might as well. I agree with Frank’s point that the delineation from existing work could be done better. It shouldn’t be too hard to do this (see also above). Changing the analysis time window to include the missed 1986 polynya would of course be great but likely be a lot of work and require a complete redo of pretty much everything. Maybe a compromise is to reference Frank’s unpublished analysis here (Is the discussion in TCD citable?), acknowledge the sensitivity of some results to the time window selection, and discuss the effect of the “missed polynya” on the conclusions. In my view, the existence of a 1986 polynya reinforces the conclusion that sea ice thickness change had little to do with the 2018 Polynya but may qualify the conclusion about the increasing frequency in the last decade (though one Polynya in 1986 doesn’t necessarily kill this). Just an idea to consider.

Nice work

Axel Schweiger