May 27, 2022

Dear Editor and Reviewers

We appreciate very constructive comments and feedbacks, which greatly helped us to improve the manuscript.

We addressed the reviewer's comments points by points and have attached a response to the reviewers.

In addition, along with the revision process, we have made additional changes in this revised version of the manuscript as following:

- The title is revised by omitting "over".

- NSIDC data are updated with CDR of Passive Microwave SIC, Version 4, which are recently released (after we submitted the first manuscript which used the version 3 data). Note that Arctic pole hole is now filled by spatial interpolation.

- The analysis of winter polynyas is expanded for winter seasons from December to March, 1979-2020. As the reviewer suggested, the winter polynya in December 1986 is included in the revised manuscript.

- The color scheme in every figure is revised for color vision deficiencies.

Thank you for your consideration of this manuscript.

Sincerely,

Younjoo Lee

[Reviewer 1]

The paper describes the performance of the fully-coupled Regional Arctic System Model (RASM) with respect to the simulation of polynya events north of Greenland. A 42-year long simulation (1979-2020) is analysed in combination with satellite products and weather station data. Additionally, two ensembles are generated by forcing RASM with output from the Community Earth System Model (CESM) Decadal Prediction Large Ensemble (DPLE) simulations. The two ensembles, initialized in December 1985 and December 2015, are investigated with respect to precondition of winter polynya events. The paper describes a nice application of dynamical downscaling. However, the paper needs major revision mainly because of two main
points of criticism:

[R1-A] It is not immediately clear what the added value of this paper in comparison to the papers of Moore et al. (2018) and Ludwig et al. (2019) is. In both of the latter papers sea ice-ocean models (PIOMAS in case of Moore et al. and NAOSIM in case of Ludwig et al.) are used to analyse the polynya event in more detail as possible with observations alone with almost identical findings (e.g. that preconditioning has no effect on the polynya event in 2018). I suggest to revise the manuscript carefully to make clearer the scientific added value of this study.
→ Moore et al. (2018) focused on how the polynya in February 2018 occurred and Ludwig et al. (2019) addressed what processes were involved in it. They both used an ice-ocean model to study it, which means that the models are prescribed with reanalysis or gridded products on every grid cell. On the other hand, RASM is a fully-coupled high resolution regional model, which allows us to further study interactions between ice, ocean, and atmosphere. In addition, we have investigated every winter polynya event since 1979, which additionally includes polynyas in February 2011 and 2017 and December 1986 in this revised manuscript. Moreover, since RASM is a fully-coupled model (where ocean, atmosphere, and sea ice fields are predicted every time step), it allows us to estimate the number of polynyas that would occur under the observed level of climate warming in each simulation in both ensembles, 30 years apart. It is found that simulated polynyas would occur as long as certain atmospheric conditions are met. This implies that an initial condition of SIT (or decline of SIT due to forced climate warming in the study region) is not a critical factor in occurrence of such polynyas north of Greenland.

[R1-B] Unfortunately, winter is defined in this study from January to March excluding December. If December would have been included the authors would have not missed the polynya north of Greenland in December 1986– Moore et al. (2018) missed it as well because they concentrated on February only (see plot below based on own unpublished analyses). Inspection of the wind field in December 1986 north of Greenland in reveals a northward wind anomaly of almost the same strength and duration as in February 2018. However, there is a dramatic difference. While the occurrence of the 2018 polynya coincides with a sudden stratospheric warming (SSW) event a few days earlier (and associated with a strong decrease in the NAO) the 1986 event does not show any SSW nor any strong NAO change.

→ We thank the reviewer for this suggestion and accordingly have expanded our analysis of winter polynyas to include December, which was missed in Moore et al. (2018). Hence, in the revised manuscript, the winter is defined from December to March and thus the December 1986 polynya is added and thoroughly examined in the subsection 4.2.2 and Fig. 9. In addition, we found that satellite SIC was below 90% in some other years: December 1984 and 2002, but they are excluded because dynamic sea ice transport as defined in this study is too small to count them as polynyas (Fig. S5). Moreover, we further investigated RASM-DPLE ensemble simulations including December, which in turn shows that more polynyas were produced when sea ice was thicker in 1985-1995.

RASM hindcast simulations reproduced the polynya in December 1986 as it is observed in satellite measurements (Figs. 9a and 9b) and confirmed it is a latent heat polynya (Fig. 9c). Although the wind in December 1986 was as strong as in February 2018, its duration was shorter (Fig. S6). If the wind in December 1986 was similar to the wind in February 2018, it is expected that the polynya would be comparable to the observed. As the reviewer reported, all the polynyas except one in February 2018 occurred in non-SSW winters, but we found that there was a link with an AO reversal (Figs. 3a and 9e).

Beside two major points of criticism I listed below a number of points the authors are ask to take into account in the revision. The importance of my suggestions is indicated by minor/major in front of each item but follows the order in the manuscript.

[R1-1]. Minor - line 49 'Introduction': Some of the citations given are pretty old and should be replaced by newer publications. One could be https://journals.ametsoc.org/view/journals/clim/34/13/JCLI-D-20-0848.1.xml
→ We added a newer publication, i.e., Ricker et al. (2021), as well.

[R1-2]. Major – line 60: Figure 1 needs some heavy revision. Panel a) is too dark. Panel b) and c) should be shown in a similar projection as a) to make the comparison easier. The rectangle in b) and c) does not compare well will panel a). Obviously RASM is not able to reproduce the large area of open water north of Fram Strait that can be seen in the observations. This should be discussed in the text and reasons for the deficit should be given (certainly shortcomings in the vertical mixing of the ocean model). The SIT from CFRv2 in panel c) is very unrealistic. A brief discussion on the reliability of SIT from CFSv2 is necessary if the plot should be shown.
→ Fig. 1a is replaced with the VIIRS nighttime image on February 25th, 2018, after brightened, so that we can compare overall open water areas on the same day in the northern Greenland as well as north of Svalbard. Due to the nature of satellite images, we cannot show the whole Arctic. But Fig 1 rather emphasizes how unrealistic sea ice condition is in CFSv2. Even though the RASM simulation relies on the downscaling of CFSv2 atmospheric boundary conditions, RASM sea ice is very well represented, indicating the potential capability of regional climate models used for dynamical downscaling. We also discussed overestimation of sea ice coverage north of Svalbard where basal melting is dominant. This could be due to that ocean heat transport underestimation along the pathway of the West Spitsbergen Current. Please see the first paragraph of the Discussion.

[R1-3]. Minor - line 63 'Introduction': I was very surprised to see no hint to the SSW when Moore et al. is cited. The coincidence of the polynia with the SSW is mentioned later under 'Discussion' but I would prefer to have some statements about the possible connection to the

SSW when citing Moore et al. for the first time because this is the strongest message in that paper.
→ As suggested, we added the sentence, introducing SSW observed in February 2018 in Moore et al. (2018).

[R1-4]. Minor but important - line 124: CS2SMOS should be referenced correctly.
→ CS2SMOS data are referenced and acknowledged as suggested.

[R1-5]. Minor – line 177: Fig. 3 is referenced before Fig. 2. Check the order of the plots. It makes the manuscript unnecessary complicated to read.
→ Fig. 2 is already introduced in the last paragraph of introduction.

[R1-6]. Major – line 179: "… to early March is captured well …". I disagree with the statement (see comment below – line 216).
→ This sentence is about near-surface air temperature variability in the RASM simulation which captures it well as shown in Fig. 3.

[R1-7]. Major – line 216: "The RASM's realistic representation of the polynya…". What seems to be realistic is the size of the polynya but not the location which is very disappointing for a downscaling system. Fig. 2 shows very convincing that the polynya is located too far to the west – the largest fraction of the polynya is located in areas where CS2SMOS shows thicknesses of more than about 1m! Reasons for the mislocation of the polynya should be discussed. In Ludwig et al. a sound estimate of the size of the polynya is given (about 600.000 km2 in maximum) but the size of the polynya in RASM is not compared to this number. This should be done! Instead modelled volume growth rates are discussed for which no observation analog exists (CS2SMOS based estimates are definitely to uncertain given the very coarse resolution). In Ludwig et al. thermodynamical growth rates based on simple estimates are given which should be discussed together with the estimates from RASM.
→ We acknowledge that RASM has a smaller polynya and a more westward position than published observations. We also have made this difference clearer in the revised manuscript. When RASM is used for dynamically downscaling, atmospheric forcings are prescribed only along the lateral boundaries and nudged at approximately the 500 hPa level and above. Hence, surface atmospheric forcing is predicted every time step. Although RASM near surface wind fields agree well with the reanalysis, slight discrepancies in wind direction or magnitude near the study region may shift the center of the polynya more westward. At the same time, it should be noted that CS2MOS SIT is a 7-day mean SIT, not daily. Hence, the direct comparison between them is not straightforward.

Ludwig et al (2019) estimated the size of the of polynya (a maximum extent of about 60,000 km^2) based on the satellite SIC, although there are large uncertainties between algorithms, which makes the comparison less straightforward. For example, MODIS SIC could produce a polynya size that is half of the current estimate (or about 30,000 km^2). RASM estimated the polynya size based on SIT less than 10 cm, which gave a maximum size of 13,000 km^2, but if the open water area is less than 25 cm of SIT, the maximum size becomes 29,400 km^2 (half the size of Ludwig et al. (2019)'s estimate). Also, the RASM integrated thermodynamic ice growth (53 km^3) is larger than their study (33 km^3).

[R1-8]. Minor – line 232: "… removal due to the polynya …" Not the polynya is removing the sea ice but the winds are removing the sea ice and forming the polynya.
→ It is revised as "ice removal during the polynya formation period"

[R1-9]. Minor – line 237: I do not understand what should be learned from whole subsection.
→ This subsection provides the additional information on how RASM southerly-southeasterly winds contribute to sea ice divergence and thus polynya formation using the SOM analysis. Because the RASM simulation is not forced by reanalysis products, we need to make sure that RASM atmospheric winds are well represented during the polynya period (Fig. 6), and they are confirmed by the ERA-Interim reanalysis wind fields (Fig. S2).

[R1-10]. Major – line 260: The whole subsection 4 might need a revision in the light of the strong wind event in December 1986 mentioned above.
→ We added another subsection for the December 1986 polynya and described how it was developed after thorough analysis (Fig. 9; ice melting, wind pattern, anomalous warming with AO index, dynamic ice volume tendency, and turbulent heat flux over the polynya region). Similar to the recent event in 2018, the strong southerly wind was involved (Table 1), but its duration was shorter (Fig. S6). Hence, this suggests that the size of polynya was smaller than the one in February 2018, as the satellite data indicate in terms of mean SIC in the region (Figs. 7 and S4). Turbulent heat flux was also lower (Table 2).

[R1-11]. Major – line 367 'Discussion': Obviously, the whole subsection needs reformulation after inspection of the December 1986 wind/polynya event.
→ After thorough analysis of satellite SIC in December 1979-2020, as the reviewer pointed out, we included the missing 1986 winter polynya in the revised manuscript. It is described in the new subsection (4.2.2). In addition, we have inspected RASM-DPLE ensemble simulations including December polynyas as well. It turns out that there are more December polynyas in the 1985-initialized runs than in the 2015-initialized ones. Text has been added to the manuscript to address the inclusion of the December polynyas and the additional conclusions that are made with their additions to the study.

[Reviewer 2]

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.
→ We appreciate your comments. We have expanded our analysis to December and the polynya in December 1986 is now examined in the revised manuscript (see also [R1-B]). This latent heat polynya was also produced by southerly winds as strong as the one in February 2018, but its wind duration was relatively shorter. Hence, the polynya in December 1986 is the second largest one. Although the revised manuscript has become a little lengthy, there are no major changes in terms of findings and conclusions.

Details:
[R2-1] 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.
→ This is now mentioned in the beginning of the discussion.

[R2-2] 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.
→ It is actually the other way around. When polynya events were observed during 2010s (i.e., February 2011, 2017, & 2018), the daily averaged satellite SIC was dropped below 90% over the study region. Hence, we applied this threshold to detect any additional winter polynya events. The statement was revised accordingly.

[R2-3] 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 SOM other paper?
→ We used SOM mainly to extract spatial patterns instead of using mean or anomaly fields because of a large size of RASM output: 90 days times four 6-hourly output.

[R2-4] 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 5 is mentioned before Figure 6 in the subsection 4.1.2. Also, as the reviewer 1 suggested, Fig. 5 is revised (including color bar) to show the north of Svalbard to discuss the discrepancy of sea ice condition in February 2018; RASM overestimated its cover (see the 2nd paragraph of the discussion).

[R2-5] 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. 6 is revised by increasing the size and the study area is zoomed in as well. The key part is to show how sea ice responds (in terms of ice divergence and convergence) to wind direction/intensity in this region.

[R2-6] 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)?
→ We would like to show that daily mean SIC for the region defined in Fig. 4a from the entire satellite records and determine potential polynya days rather than ones that we already know. We found that those polynya days coincide with when the regional mean SIC is below 90%. The standard deviation indicates spatial variability of SIC in each day. For example, if SIC is spatially homogeneous in a given day, then standard deviation is almost zero. On the other hand, when a polynya occurs, the spatial mean SIC drops down and standard deviation goes up. The figure captions of Figs. S3 and S4 are revised for clarity.

[R2-7] Line 275: Polynya periods… defined as.
How does this interact with your previous definition of 90% SIC. Needs some clarification.
→ Text has been added to clarify that the polynya period is based on the RASM simulation when dynamic sea ice loss (i.e., DVT is less than -10 km^3/day) for more than 3 days. The criterion stated above (i.e. 90% satellite SIC) was a sea ice condition, regionally averaged, when we found polynyas over the region. The corresponding statement is also revised.

[R2-8] 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.
→ As suggested, a new subsection was assigned to this paragraph. However, we introduced the polynya in December 1986 in the following subsection and made a few changes in the beginning of the paragraph because the polynyas in the 2010s are not unique anymore. This paragraph is

also revised by adding the reason why we used two ensembles at the end. In addition, the last paragraph of the new section 4.4. is revised to confirm the hypothesis: the role of southerly winds. Finally, we added a comment about uncertainty of causes changes in wind pattern.

[R2-9] 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.
→ We agree to the idea that it is not sufficient enough to bring large-scale atmospheric connection. Hence it is removed.

[R2-10] 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?
→ This sentence is removed due to uncertainty.

[R2-11] 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.
→ As the reviewer 1 suggested, we decided to include December analysis in our analysis of satellite SIC, RASM hindcast, and RASM-DPLE runs. As you already pointed out, the polynya event in December 1986 further supports the idea that sea ice decline plays little role in more frequent occurrence in recent years. There are some changes in the manuscript because the polynyas in 2010s are not "new" any more, but overall structure, findings, and conclusions are basically the same. Interestingly, RASM-DPLE simulations produced more December polynyas in 1985-1995 than in 2015-2025.