

Review on “Causes and Evolution of Winter Polynyas over North of Greenland” by Younjoo J. Lee, Wieslaw Maslowski, John J. Cassano, Jaclyn Clement Kinney, Anthony P. Craig, Samy Kamal, Robert Osinski, Mark W. Seefeldt, Julianne Stroeve, Hailong Wang.

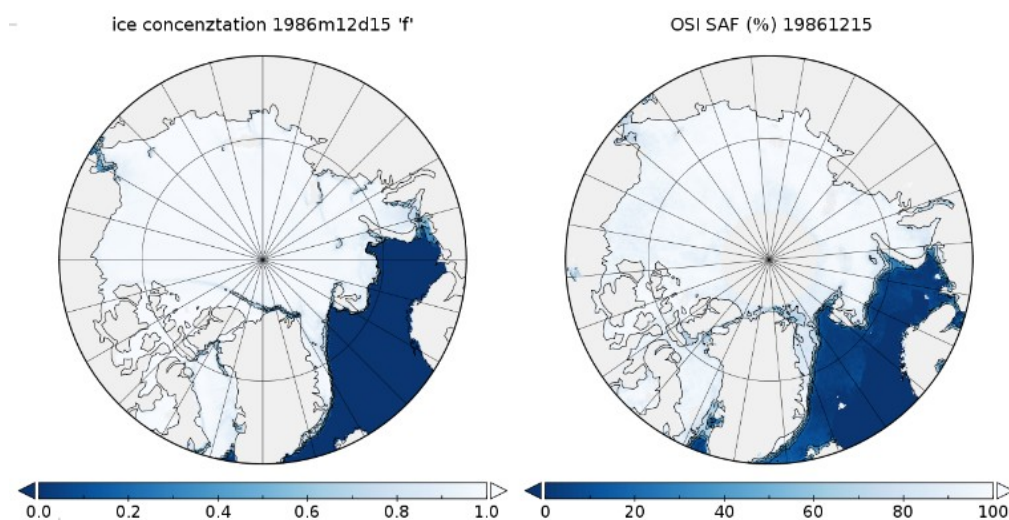
The paper describes the performance of the fully-coupled Regional Arctic System Model (RASM) with respect to the simulation of polynya events north of Greenland. A 42-year long simulation (1979-2020) is analysed in combination with satellite products and weather station data.

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.

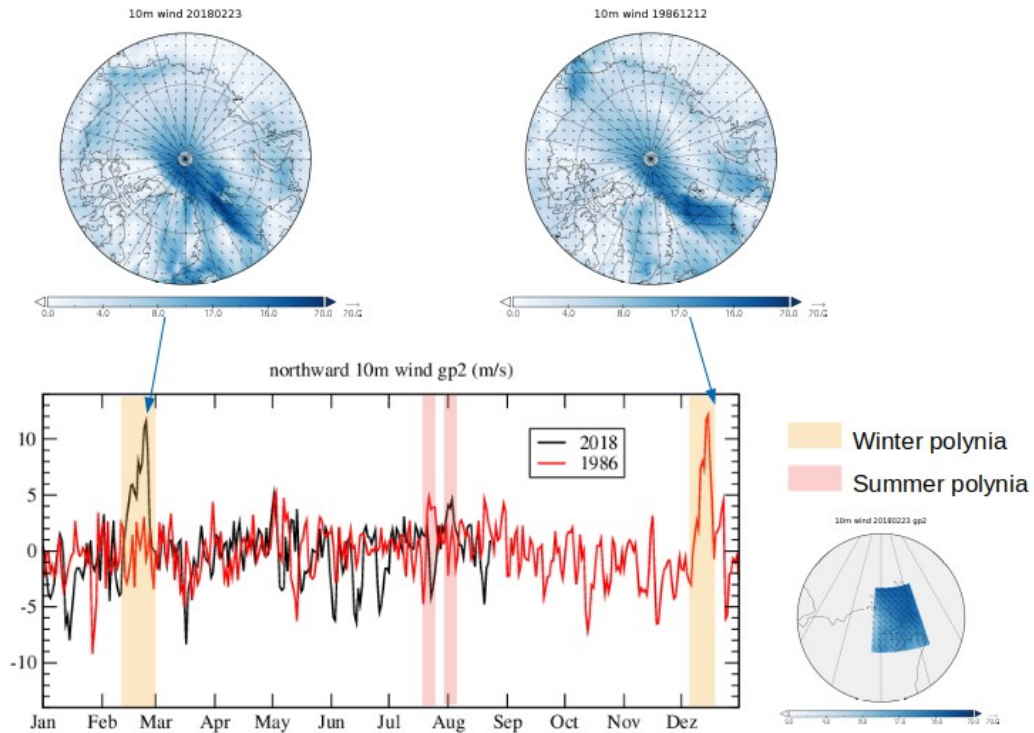
The paper describes a nice application of dynamical downscaling. However the paper needs major revision mainly because of two main points of criticism:

(1) 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. (2029) 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.

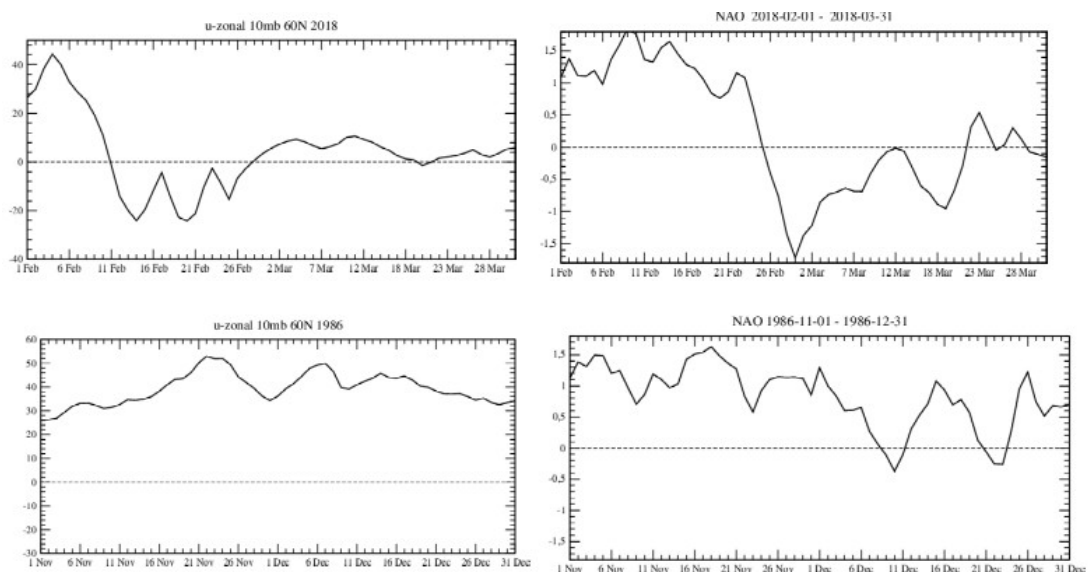
(2) Unfortunately, winter is defined in this study from January to March excluding December. If December would have been included the authors would have not missed the polynya north of Greenland in December 1986 – Moore et al. missed it as well because they concentrated on February only (see plot below based on own unpublished analyses). The plot shows the ice concentration as modelled by NAOSIM (left panel) and as observed by satellite observations (OSI SAF – right panel) on 15th of December 1986



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 (plot below)



However, there is a dramatic difference. While the occurrence of the 2018 polynia 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 (plot below - left: u-zonal 10mb in February 2018 (top) and in December 1986 (bottom); right: NAO) (based on NCEP/CSFR/CSFv2).

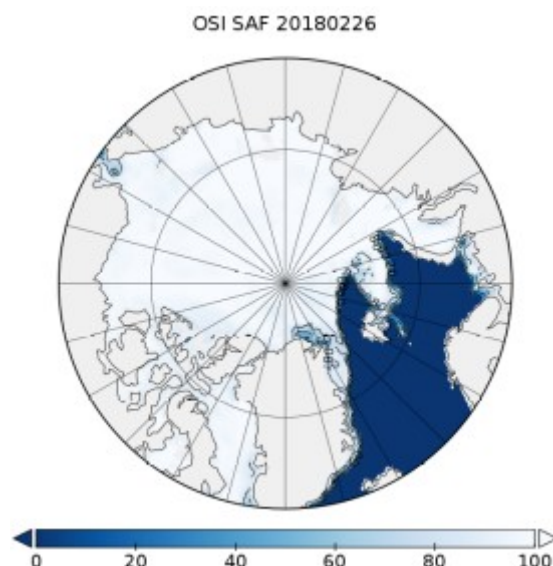


RASM might be an ideal tool to analyse this event in 1986 as well with respect to the processes in the atmosphere. (The reason why the own research presented above was never published is that we had no appropriate fully-coupled model at hand to perform a thorough analysis in the atmosphere (and probably to perform some sensitivity studies with such a model)).

Beside this 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.

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>

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 with 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 (cmp. plot below). 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 CFRv2 is necessary if the plot should be shown.



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.

4. Minor but important - line 124: CS2SMOS should be referenced correctly. From meereisportal.de:

For all CryoSat-2/SMOS data, please

1) include the following phrase into the acknowledgment:

The merging of CryoSat-2 und SMOS data was funded by the ESA project SMOS & CryoSat-2 Sea Ice Data Product Processing and Dissemination Service and data from DATE to DATE were obtained from <https://www.meereisportal.de> (grant: REKLIM-2013-04).

2) refer to: Ricker, R.; Hendricks, S.; Kaleschke, L.; Tian-Kunze, X.; King, J. and Haas, C. (2017), A weekly Arctic sea-ice thickness data record from merged CryoSat-2 and SMOS satellite data, *The Cryosphere*, 11, 1607-1623, doi:10.5194/tc-11-1607-2017.

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.

6. Major - line 179: "... to early March **is** captured well ...". I disagree with the statement (see comment below - line 216).

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.

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. Please revise.

9. Minor - line 237: I do not understand what should be learned from whole subsection.

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.

11. Major - line 367 'Discussion': Obviously the whole subsection needs reformulation after inspection of the December 1986 wind/polynya event.