The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC3
https://doi.org/10.5194/tc-2021-250-RC3, 2021
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Comment on tc-2021-250

Anonymous Referee #3

Referee comment on "Evolution of the Amundsen Sea Polynya, Antarctica, 2016–2021" by Grant J. Macdonald et al., The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2021-250-RC3, 2021

I was asked by the editor to provide a 3rd review of the manuscript "Evolution of the Amundsen Sea Polynya, Antarctica, 2016-2021" by Macdonald et al. submitted to the Cryosphere 2021. Two reviews were available with one recommendation to reject and the other as a major revision.

The authors aim to investigate the evolution of the Amundsen Sea Polynya, Antarctica by the means of SAR images, AMSR2 ice concentration and ERA 5 reanalysis data. The study area is motivated by the highest primary productivity and key oceanographic significance. The time period 2016-2021 is very limited by the availability of Sentinel-1 SAR images. Specifically, the aims are to analyse qualitative changes in the characteristics, the summer polynya area and the winter polynya area and ice production.

The authors do not precisely identify the novel aspects of their investigation but I think it is mainly the use of Sentinel-1 data. This is in principle a good and interesting approach to investigate the polynya dynamics in the special Amundsen Sea environment. However, the limitations due to the sparse temporal sampling are not adequately discussed, and the knowledge gained is not presented in a comprehensible manner. Unfortunately, the quality of the supplementary video is only poor and does not support the claims. If the visual SAR video/image analysis were carried out by showing more example results, I would rate the present manuscript much better.

The scientific quality of the study is only poor. I agree with the fundamental concerns of reviewer #1. In addition to the concerns about neglecting the effect of thin ice thickness there is an inconsistency with the applied data sets. The high resolution AMSR2 ice concentration used for the manuscript is different from the low resolution OSI-SAF/OSTIA ice concentration used in the ERA5 reanalysis. This means that there are likely large inconsistencies for the calculation of heat fluxes when the low resolution data do not resolve the open water polynya. Unfortunately, there is no error assessment or sensitivity analysis.

I do not fully understand the study logic. I would have expected a comparison between the passive microwave and SAR derived polynya areas. The scalar wind speed is of very limited use to describe the polynya which can be seen in Fig. 8. The surprisingly low correlation indicates that the wind speed explains only very little of the polynya area variability. A more insightful approach would have been the usage of wind direction like outlined in Haarpaintner et al. (2001) who compared SAR-derived polynya width/area with model based results.

The literature regarding the impact of the Amundsen polynya is not well balanced, i.e. the impact for carbon sequestration is emphasized but this seems to be controversial (Lee et al., 2017). One interesting question is how to define a polynya during Summer, in particular when there is only surrounding open water.

In summary, I concur with review #1 and suggest to reject the manuscript but encourage the authors to resubmit their work after a fundamental revision. The aspect of the SAR image analysis would be very interesting when compared with the passive microwave data. The gain in knowledge has to be identified and the main hypotheses have to be worked out and supported by evidence.

Lee et al. (2017) https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017GL074646

Haarpaintner et al. (2001)
https://agupubs.onlinelibrary.wiley.com/doi/pdf/10.1029/1999JC000133