

The Cryosphere Discuss., author comment AC2
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Reply on RC3

Grant J. Macdonald et al.

Author comment on "Evolution of the dynamics, area, and ice production of the Amundsen Sea Polynya, Antarctica, 2016–2021" by Grant J. Macdonald et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-51-AC2>, 2022

Thank you for taking the time to review our paper. We include responses to each point below.

Evolution of the dynamics, area and ice production of the Amundsen Sea Polynya, Antarctica, 2016-2021 by Macdonald and others.

This study looks at the dynamics of the Amundsen Sea Polynya (ASP) using a combination of active and passive microwave observations. While the results present potentially useful information about the dynamics of the ASP, I agree with Reviewer #1 and Reviewer #3 (first draft) that additional scientific quality assurances need to be taken. The major points raised by those Reviewers would only serve to improve the quality of the manuscript so I was surprised they are rebutted and not implemented. The major problem with only using video is the statements about processes/characteristics cannot be supported clearly with evidence and therefore subject to miss-interpretation. This paper is full of casual statements that maybe true but lack quantitative support which is not scientific. The threshold approach for polynya area is also problematic.

Regarding the qualitative analysis of the video: of course with observations, whether of optical imagery, SAR or from the field, there is the chance of mis-interpretation. However, this does not mean expert interpretation of observations is not valuable without quantification. Visual interpretation of various forms of imagery and field observations has long had a place in physical and geographic sciences. Furthermore, it is not without evidence, the imagery is the data and evidence. They are not 'casual statements' but conclusions from hours of analysis of the imagery, and examples are provided in the main paper of processes (e.g. polynya events, back-flow, secondary polynya formation), as well as in the time-lapse video. It is not clear that the key claims of the video analysis (such as that polynya events occur throughout winter, that ice 'backflows', secondary polynyas occur, the role of topography in their location) require some form of quantitative result to be supported when they are observable. Visual interpretation of SAR imagery has formed an important part of numerous sea ice studies, some of which are referenced within.

Quantifying these observations would be a huge task, if indeed possible – and in some cases it is not clear exactly what about the process would be quantified in in order to provide further evidence for them, within the scope of this study.

Automated quantification of polynya states (e.g. area, frazil ice cover) using SAR is an active area of challenging research that groups are working on, typically involving machine learning techniques. To disregard what can be seen in the imagery because it cannot currently be quantified would mean missing the opportunity to take advantage of an excellent observational tool. We are clear that the SAR analysis is qualitative and descriptive, but we believe, as stated by Reviewer 2, that importantly this contributes relevant information to the community and therefore merits publication.

(Note Reviewer 1 of this draft is the same reviewer as one of the reviewers on previous iteration)

I would encourage the author's to actually implement the suggestions of the previous Reviewers also taking into consideration my comments:

A very large part of the critiques (and associated suggestions) were based on a different understanding of what we mean by 'open polynya' in the winter. We have attempted to clarify that in the response and pointed out that our definition of an 'open' polynya in winter is already established in the literature, but pledged to make it clearer in the paper by instead referring to 'active' polynya. The other primary criticism was of the qualitative nature/visual interpretation element of the analysis. We have outlined that we dispute the idea that qualitative analysis and visual interpretation lacks scientific value and highlighted the unfeasibility of quantifying most of what we qualitatively describe in the SAR. We have implemented numerous other changes in response to all the previous reviewers, and suggested others in response to Reviewer 1's comments on the current iteration. Regarding suggestions we have not implemented, we have already explained in our responses why we think they are unnecessary or are based on a misunderstanding of something we have made/will make clearer when revising.

Reviewer 1 of this iteration makes only one actual suggestion – to compare our quantitative (SIC-derived) polynya-area estimates to results from lower-resolution SIC products. We suggested comparing to actual observations of the polynya from SAR is a better form of checking and validating.

- The problems with passive microwave data underestimating thin ice (and ponded ice) are well-known so a threshold approach is not ideal. I think a better approach would have been to construct the time series of open water area from PM rather than polynya area based on a threshold. This is done nicely in Moore et al. (2021; 10.1029/2021GL095099).

For readers not familiar with the above-mentioned Moore paper, it uses the same SIC product as in our paper to present/analyse the opening of a polynya in the Arctic in May 2020. It also includes a plot of 'open water' from this data in May 2020 and the mean of each May 2003-2021. They do not detail how they define 'open water' but they seemingly classify it as when SIC = 0.

Their use of this data supports our use of the same dataset for polynya identification, at least in summer. We would say that their approach also effectively uses a threshold to define 'open water', just that the threshold is 0 rather than ours of 70% to identify 'open/active polynya'. Based on checking the SIC data against direct observations of the polynya in the SAR imagery, we found a threshold of 70% to be better at estimating the 'open/active polynya' than 0 for our case. A threshold of 70% has also been used in several previous studies referenced in our paper (e.g. Parmiggiani, 2006; Morelli & Parmiggia 2010, 2013; Preußer et al., 2015; Cheng et al., 2017; 2019). In a revision we could include a

figure in the supplementary material showing the SIC values (i.e. with no threshold applied) for an open/active polynya, alongside a SAR image.

We will also clearly acknowledge that passive microwave data underestimates SIC where there is thin ice. (But as noted in the paper and in responses, an open/active winter polynya is expected to include thin ice that forms almost immediately upon opening, and therefore classification of these areas as open/active due to this issue and associated low SIC values is not likely to lead to mis-classification – as shown by our comparison with SAR)

Additionally, we will reference Moore et al. in a revision.

- Polynya or open water area can indeed be better estimated by SAR but some attempt to do this quantitatively and consistently needs to be made to illustrate variability and ensure reproducibility. There is no problem manually extracting polynya or open water area from SAR imagery but this should be done carefully and ideally in time series image analysis format. Further, the imagery must be pre-processed (i.e. calibrated and corrected for incidence angle). Not doing these basic things comes across as scientifically lazy and also introduces errors. Moreover, inclusion of the backscatter scale **IS** the standard in the literature with respect to ice monitoring.

We suggest that we could manually delineate and calculate the polynya area in sample SAR images for comparison with our automated approach, for the purpose of validating our automated approach using SIC data. For example, compare the areas from SAR and SIC for the case study in Fig. 2. Manually delineating all the SAR images would be an unfeasibly laborious task for this paper, and as mentioned above, an automated identification approach with SAR is currently beyond our capabilities. I am not sure I understand what exactly is meant by 'should be done...in a time series image analysis format'.

SAR imagery was pre-processed. All Sentinel-1 imagery Google Earth Engine is pre-processed (i.e. those in the time-lapse), and images presented in figures and downloaded for further analysis were pre-processed in SNAP. Although we do state images were pre-processed in SNAP (lines 160-163) were not clear about the images that were processed in Google Earth Engine – we will amend this in a revision.

We will include a backscatter scale showing the decibels of SAR images in a revision.

- Studies that use passive microwave to identify polynya area (or open water) together with SAR need to at least quantitatively compare the two estimates. In the current version of the paper this is not very rigorous and at the very least should also be placed on Figures 5 and 6. Timing of formation/closure should also be considered.

Regarding a quantitative comparison we repeat from our response to point 2:

“We suggest that we could manually delineate and calculate the polynya area in sample SAR images for comparison with our automated approach for the purpose of validating our automated approach using SIC data. For example, compare the areas from SAR and SIC for the example in Fig. 2. Manually delineating all the SAR images would be an unfeasibly laborious task for this paper, and as mentioned above, an automated identification approach with SAR is currently beyond our capabilities.”

Additionally, we could add an error estimate to Figures 5 and 6 based on this comparison.

Best regards,

Dr Grant Macdonald, on behalf of all authors.