

The Cryosphere Discuss., referee comment RC2  
<https://doi.org/10.5194/tc-2022-51-RC2>, 2022  
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## Reviewer comments on tc-2022-51

Anonymous Referee #2

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Referee 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-RC2>, 2022

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### GENERAL COMMENTS

Since The Cryosphere is meant to be an "open" journal, I'll point out that this manuscript is a resubmission (previously submitted to the same journal), that I've reviewed the manuscript before, and that my earlier review is available at:  
<https://doi.org/10.5194/tc-2021-250-RC2>

I would also like to point out that remote sensing isn't my expertise. Therefore, I cannot comment on aspects related to e.g. SAR imagery or AMSR2 algorithms. I rely on the other reviewers (and on the editors) to evaluate these specific aspects.

My "general comments" from the earlier review still hold, so I'm repeating them here:  
"I think the manuscript's topic fits nicely within the scope of the journal. As far as I know, this manuscript is unique (i.e. original) in its detailed description of Amundsen Polynya, providing far more information than earlier publications. The study is also scientifically significant given the regional importance of the Amundsen Polynya, either in terms of sea ice production (4th in Antarctica) or for its role in phytoplankton production and atmospheric CO<sub>2</sub> uptake."

RECOMMENDATION: The authors have addressed all but one of my earlier comments, so unless my co-reviewers identify major flaws that I've missed, I think the manuscript will be acceptable for publication after minor revisions (see "Specific Comments", below).

SPECIFIC COMMENTS: MINOR

(1) About Section 4.4, "Wind and polynya area": I think the existing analysis of Sec.4.4 is very basic, and that a lot more could be done to establish the relation between the winds and the polynya area. In my earlier review, I was making a number of suggestions: (a) Rather than limiting your analysis to wind velocity, try to incorporate information about wind direction; (b) see if the *\*time-derivative\** of the polynya area is better correlated to winds than the area itself, (c) see if the *\*cumulated\** wind correlates better with the polynya area than the wind itself (this metric works well in the context of coastal upwelling; see <https://doi.org/10.1029/2006GL027149> for an application.)

Such additional analyses are not necessary for the paper to be worth publishing, but I do believe the authors are in a unique position to discover something interesting about winds and the area of the Amundsen Polynya, and it would worth giving it one more try.

(2) Line 258: The sentence on this line needs to be flipped for consistency with the revised Eqs.2,3. It should be:

"The temperature of the water surface ( $T_S$ , in K), was assumed to be at the freezing point of seawater ( $T_0$ , in K) which was calculated following..."

(3) Another reviewer commented that the manuscript was "too qualitative". I do agree that the present manuscript is, to a large extent, descriptive. On the other hand, I believe that scientific studies should first and foremost be *\*relevant\** to the community, regardless of where they fall on the descriptive-to-quantitative scale. The present manuscript is definitely relevant to my own research projects, and I would cite it in a heartbeat if it were to be accepted by the journal.