

Reviewer 1 point-by-point reply

All replies from the authors are written in **dark red font**, whereas the original text from the comments is in black.

I thank the authors for thoughtfully addressing the concerns raised in the first round of reviews. While I am generally satisfied with the revised manuscript, which has substantially improved, I think there are several issues that need to be addressed before publication can be recommended. My main issue is with the revised Introduction section, where the authors attempt to summarize the oceanographic literature on polynya formation. As I detail below, the authors introduced statements that I view as misleading and inaccurate.

That said, I think the issues mentioned above are relatively minor and the scientific core of the paper remains valuable. While this new sea ice thickness retrieval comes with many caveats, the authors are clear about its limitations and demonstrate that it provides information that is distinct and independent of existing sea ice area information. Future work is needed to validate these retrievals and quantify their uncertainties, but this study is a good start.

Detailed comments:

- Title: This title is fine but many people in the ocean community understand "Weddell Sea Polynya" to mean large open-ocean polynyas that span *most* of the Weddell Sea, which this paper does not analyze. I would suggest a more general title, such as "Analysis of open-ocean polynyas in the Weddell Sea using SMOS-SMAP apparent sea ice thickness retrieval"

Thank you for the suggestion but we decided to stick with our original title. While the Weddell Sea polynya is often synonymous with the Weddell polynya, it is to our understanding the latter name is what was first and is most commonly applied to the large polynya events of the 1970s (Carsey, 1980). At the same time, our current title is without an article and therefore aims not to reference any particular polynya event.

- Line 2 (abstract): Related to the above comment, this definition of "Weddell Sea open-ocean polynyas" does not quite align with definitions found in the literature. While open-ocean polynyas are often found near the Maud Rise seamount, they are not exclusive to this specific region. Perhaps an opening sentence like the following would suffice, "The Weddell Sea is known to feature large openings in its winter sea ice field, otherwise known as open-ocean polynyas...."

The first 3 sentences are now written as follows: "The Weddell Sea is known to feature large openings in its winter sea ice field, otherwise known as open-ocean polynyas. An area within the Weddell Sea region that has repeatedly featured open-ocean polynyas in the past is that which encompasses the Maud Rise seamount. Within this area, after 40 years of intermittent, smaller openings, a larger, more persistent polynya appeared in early September, 2017, and remained open for approximately 80 days until spring ice melt."

- Lines 3-4 (abstract): "2017, however, is far from the only year that the imprint of a polynya can be identified at this location." This is an odd sentence considering the previous sentence states that these polynyas have intermittently appeared over the past 40 years. Perhaps the authors wish to communicate that their new estimates of sea ice thickness reveal periods of the substantial thinning that are not apparent in existing observations of sea ice area/concentration.

The sentence has been changed to: "In this study we present proof that polynya-favourable activity in the Maud Rise area is taking place more frequently and on a larger scale than previously assumed."

- Lines 23-34: I would delete "surprisingly".

The sentence has been changed as indicated.

- Lines 26-28: I would suggest referring to these openings as "open-ocean polynyas". I think the use of lower-case "p" in the term "Weddell Sea polynya" only makes things more confusing.

Thank you for the suggestion. Since in the first paragraph of the introduction we define what we mean by that term, we believe it's sufficient as is. Changing the "P" to lower-case was suggested by reviewer 3 of the original submission in an effort to further standardize the nomenclature without referring to any one event. We believe for this study, including the Weddell Polynya and Maud Rise Polynya under a common name is necessary, and simply open-ocean polynyas is too vague. Meanwhile, open-ocean polynyas in the Weddell Sea or open-ocean polynyas near Maud Rise are too long to repeat regularly.

- Line 29: "The Weddell Sea polynya is an anomalous opening in sea ice that is generally classified as an open-ocean polynya" See above comment.

See the above reply.

- Line 32: "ocean processes" is rather vague. I prefer the original description that mentions the upwelling of warm water.

The sentence has been changed to: "An open-ocean or 'sensible heat' polynya is distinguished from the coastal 'latent heat' polynya by being maintained and opened by upwelling and/or mixing as opposed to wind-driven ice advection."

- Lines 32-34: "The main mechanism that preconditions the Weddell Sea polynya is described by Martinson et al. (1981) as destratification of water masses leading to a raised pycnocline. The raised pycnocline compresses the winter surface mixed layer that is made dense through heat loss and brine rejection from ice formation, resulting in density overturning." This explanation is rather muddled and arguably incorrect. I would delete this sentence since the one that follows gets right message across.

The text has been modified as follows: "The main mechanism that preconditions the Weddell Sea polynya is described by Martinson et al. (1981) as the winter surface layer becoming dense through heat loss and brine rejection from ice formation, resulting in density overturning (Martinson et al., 1981; Martinson and Ianuzzi, 1998) which deepens the mixed layer and initiates deep convection. Deep convection and heat

ventilation into the mixed layer are thought to be primary causes for the Weddell polynya (Martinson et al., 1981; de Steur et al., 2007; Wilson et al., 2019; Cheon and Gordon, 2019)”

- Lines 37-45: This section contains several statements that are confusing, misleading, and at times inaccurate. This is perplexing since the section was mostly fine in the original submission. To give a few specific examples: I don't understand what is meant by "...destratification of water masses leading to a raised pycnocline..." or the idea that a raised pycnocline "compress[es]" the mixed layer. Additionally, the authors appear to use "weak stratification" and "lack of stratification" inter-changeably, which are very different.

Lack of stratification has been corrected to lack of strong stratification. For the other remarks, see the modified text above.

Martinson et al. (1981), which the authors try to summarize, proposes that open-ocean polynyas in the Weddell Sea may be initiated when the winter mixed layer becomes sufficiently dense that it triggers deep convection, which results in the upward mixing of warm deep water. These events may be preconditioned by processes that weaken the upper ocean stratification, such as the anomalous upwelling or advection of high salinity water into the surface mixed layer. I don't think the authors need to spend more than a couple sentences reviewing these mechanisms. Like I mentioned before, the initial text was mostly fine.

The compression of the surface layer is written about in detail in the Martinson et al. 1981 (summarized in Conclusion point 3 and first proposed in the introduction part c. Convective processes), and the paper as a whole was referred to us multiple times by Reviewer 3 who emphasized the importance of original text on the polynya processes. Nevertheless this process is indeed not the primary cause for density overturning and instead more precedence should be given to the salinity increase - thus we thank you for having us correct the order of events as well as better briefly summarize the idea.

- Lines 44-53: Reading this for a second time, I think this extended discussion about Taylor columns and flow-topography interactions lacks appropriate nuance and does not add much to the paper. With my previous comment in mind, I would also suggest reducing this entire paragraph to a 1-2 sentences. For the purposes of this study, I think it is fine to simply state (with supporting citations) that interactions between the mean flow and topography preconditions the area above Maud Rise for anomalously high vertical heat fluxes, which favors thinner sea ice.

The extended description has been replaced by “Muench et al. (2001) went further to describe how interactions between the mean flow (eastern limb of the Weddell Gyre) and topography (Maud Rise) preconditions the area above Maud Rise for anomalously high vertical heat fluxes, which favors thinner sea ice.” as you suggested.

- Lines 58-59: "Direct atmospheric forcing" and "large-scale climate processes" are framed as two separate entities but the former is a subset of the latter.

The term “direct atmospheric forcing” has been changed to “localized atmospheric forcing” to be more precise. What we aim to embody with this is the interaction between the atmosphere and ice occurring in a given location, in this case the Maud Rise area.

- Lines 117-118: "Patilea et al. (2019) estimated the uncertainty at 90% SIC from SIT values up to 50%." While I get the gist of this discussion, I'm a bit confused by this line.

That was a mistake, it should be 50 cm. Thanks for pointing it out. It has now been corrected.

- Lines 123-124: "...this compromises the validity the of the sea ice thicknesses retrieved..." Typo. "the" after validity is not necessary.

The sentence has been changed as indicated.

- Lines 125-126: "While the degree by which the ice thins is difficult to quantify in terms of uncertainty, our analysis has shown that the pattern of thin ice anomalies above Maud Rise is not random nor is it identical to the distribution of low SIC areas..."

Is there a simple, quantifiable way of demonstrating this statement? I wish I suggested this in my first review, but it would be interesting to compare these SIT measurements with available in situ ocean salinity measurements or perhaps a data-assimilative model such as the Southern Ocean State Estimate. Periods of thinning should correspond to periods of mixed layer freshening. In fairness to the authors, I will not demand that they do this validation but I hope they consider doing so in the future.

That would indeed be very useful, and while it is beyond the scope of this manuscript, it will be considered for the future. Some in-situ measurements (moorings deployed in the area by the Alfred Wegener Institute) were analysed in the original master thesis that inspired this manuscript. However, this data was not sufficiently spatially coincident and therefore deemed unreliable when compared to the ASIT record. An additional sentence has been added to the outlook section to encourage further work on this necessary comparison.

Lines 294-295: "anomalous episode of sea ice" is a bit vague. How about "...episodes of anomalous wintertime sea ice loss..."?

The phrasing has been changed as indicated.

Line 321-322: "In the end, strength of the wind magnitude present above the region is most directly connected with drops in ASIT and SIC." This is an interesting finding!

Reviewer 2 point-by-point reply

All replies from the authors are written in **dark red font**, whereas the original text from the comments is in black.

Overall, I see the paper has been improved a lot, although I am not sure why they do not want to comment or discuss the Nihashi et al. 2017 and Dai et al. 2020 papers and cite them. If the method used in these two papers had problems in their opinion, it is a good place/time to say a few words, so future improvements/discussions can be made, right? I notice the English writing has been improved as well, but still need more work. For example, I went through the first part of the paper (up to page 7) and I list quite a few here. I would hope the authors to go through the entire paper and make corrections before final acceptance.

We are not opposed to the two mentioned papers. We just don't think citing them here is very relevant for our study. We are aware that substantial work was done for monitoring Antarctic coastal polynyas from space. This includes also some of our own work (Kern, S., G. Spreen, L. Kaleschke, S. de la Rosa Höhn & G. Heygster (2007). Polynya Signature Simulation Method polynya area in comparison to AMSR-E 89,GHz sea-ice concentrations in the Ross Sea and off the Adélie Coast, Antarctica, for 2002-05: first results. Ann. Glaciol., 46, 409-418.), which we also do not cite here. Importantly the open-ocean polynyas we analyse in this manuscript are different to coastal polynyas and methods used to analyse the latter might not be suitable for the former.

Line 6, to make a consistent tense through the paper, I would think “we found” should be “we find”

The sentence has been changed as indicated.

L7, change “occurring” to “occurred”

The sentence has been changed as indicated.

L16-17, change “effect” to “phenomenon”? “is” to “was”, “and similar anomalies are identified” to “but also identified”...

The sentence has been changed to: “This phenomenon, as we have shown in the 11-year SMOS record, was not unique to 2018 and was also identified in 2010, 2013 and 2014.”

L23, change “are” to “were”

The sentence has been changed as indicated.

L26, change “will refer to” to “refer”

The sentence has been changed as indicated.

L42, to cite all previous /published papers, I believe the past tense should be used. In this line and many other places please make the changes. “go further” to “went further”...

All instances of “go further” in this context have been changed as indicated.

L54, the same as comment above, “describe” to “described”

The sentence has been changed as indicated.

L55, “discuss” to “discussed”. Since this already happened, the past tense should be used. Everything you are doing, discussing and finding, should be present tense in this paper. So readers can separate this paper’s work (present tense) and previous works (past tense).

The sentence has been changed as indicated.

L63, “go further” to “went further”

The sentence is changed as indicated - see above.

L64, “hypothesize” to “hypothesized”

The sentence has been changed as indicated.

L65, “preventing stabilization” to “preventing that stabilization”

The sentence has been changed to: “Ice divergence due to strong winds enables rapid ice production and brine rejection that prevents stabilization from ice melt as wind-driven turbulent mixing entrains warm and saline water into the surface mixed layer.”

L76, add “that” before “the existence of ...”; change “conditions that although” to “conditions although”

The sentence has been changed to: “Using the ASIT retrieval over years where the polynya did not occur, we aim to identify low sea ice thickness areas that demonstrate anomalous behaviour taking place in the absence of the Weddell Sea polynya suggesting that the existence of polynya-favorable conditions, although present, is insufficient to produce the polynya.”

L77, change “are” to “is”; change “use” to “used”

The sentence has been changed as indicated.

L100, change “which” to “This”

The sentence has been changed as indicated.

L158, change “report” to “reported”

The sentence has been changed as indicated.

Figure 1, based on the 66S and 3E, I don’t see the thin ice is on top of the Maud Rise? Do you mean north of the Maud Rise?

Figure 1 caption does not claim the sea ice thinning is directly on top of Maus Rise nor does the manuscript. Looking at the introduction as well discussion we

describe why we are more likely to expect low SIC and SIT on the flanks of the rise and indeed that is what is shown both in SIC and SIT records.

L168, change “it the area” to “it is the area”

The sentence has been changed as indicated.

L174, change “shows” to “showed”

The sentence has been changed as indicated.

L175, change “there is” to “there was”

The sentence has been changed as indicated.

A message to both reviewers:

We thank you for reviewing our work for the second time and for once again helping us to improve the manuscript as a whole. Your comments and suggestions throughout the review process were both helpful and appreciated.