In the following reply, you may find your own comments marked in **gray** and the replies by the authors of the manuscript indented and in **black**.

General Reply:

> We would like to thank you for your evaluation and insightful comments. We are pleased to know that this work has the potential to benefit the field of oceanography through remote sensing. This manuscript was written first and foremost to identify what the SMOS and SMOS-SMAP sea ice thickness retrievals pick up when looking at the polynya-prone region. That said, ERA5 atmospheric reanalysis data was also analyzed in parallel leading to the secondary conclusion that corroborates past studies on atmospheric influences over the Maud Rise region.

i) Acknowledgment of relevant past studies is inadequate: One of the main conclusions from this work is that winter storms in the Weddell Sea can drive substantial basal melting, which may or may not manifest itself as a polynya (i.e. a complete melting of the sea ice cover). While the authors do present fresh evidence in support of this hypothesis, they fail to acknowledge the long history of this idea in the literature. A well known example is the field report by McPhee et al. 1996, which documents the ANZFLUX field campaign near Maud Rise during the austral winter of 1994. In this report, they describe how the passage of a series of storms led to so much basal ice melting that they abandon their field site on the ice. A more detailed assessment of the results from that research cruise is provided by McPhee et al. (1999). Additionally, a few modeling and theoretical studies have highlighted the importance of wind-driven mixing in generating basal melting in the Weddell sea (e.g. Goosse and Fichefet 2000 and Wilson et al. 2019). With regard to the 2017 polynya event, Francis et al. (2019) and Campbell et al. (2019) both provide detailed accounts of the polar cyclones and atmospheric conditions leading up to the polynya event. While the authors credited Campbell et al. (2019) for highlighting the impact of storms, they did not cite the equally relevant study by Francis et al. (2019). I will restate that this work does provide new and valuable validation of the notion that storms are an important ingredient for the generation of open-ocean polynyas. I also agree with the authors that importance of storms in the generation of polynyas are not acknowledged to the same extent as ocean preconditioning and large scale Ekman pumping. That said, many studies have argued in favor of these storms (i.e. McPhee et al. 1996, McPhee 1999, Goosse and Fichefet 2000, Wilson et al. 2019, and Francis et al. 2019) and they deserve to be acknowledged here.

> We would like to thank you for providing us with more fundamental literature on this topic that has broadened our understanding and helped formulate a better

manuscript. More importantly, we are pleased that this will lead to acknowledging the researchers that have studied this effect extensively.

ii) Description of SMOS-SMAP sea ice thickness retrieval is a bit lacking: While I appreciate the technical discussion provided in the data and methods section, I am still left with an unclear understanding of the sea ice thickness (SIT) retrieval process, its limitations, and the measurement uncertainties associated with the data product. Since similar SIT data have been available in the Arctic for several years now, the big question in my mind is why did it take so long to apply this technique to Antarctic sea ice. My understanding is that Antarctic sea ice has the added complication of snow-loading, where the weight of snow depresses the sea ice surface below sea level, leading to the formation of a snow-ice slush that has poorly defined densities. However, this issue is only briefly mentioned and immediately cast aside in the discussion of the results (line 275). I also don't fully comprehend the connection between the errors in SIT and sea ice concentration (SIC). In lines 100-111, the authors mention that the SMOS-SMAP retrieval algorithm assumes near 100% SIC and that they need to "correct" for this assumption using predefined SIT/SIC rations. For example, the authors acknowledge that 50 cm thick ice that is retrieved at 90% SIC is adjusted down to 28 cm (line 107). I am surprised by the magnitude of this adjustment and makes me question the validity of these results. What's even more puzzling is the assertion that these SIT values should not be combined with SIC to estimate ice volume. Overall, I think the methods section needs one or two additional paragraphs that give a more general (i.e. less technical) overview of the SIT retrieval process and the limitations and uncertainties associated with these estimates. I anticipate most readers will not be familiar with the details of these satellite retrievals and data processing, so this is an opportunity for the authors to educate the community on how best to use and interpret this valuable dataset.

With regard to the sea ice thickness (SIT) retrieval, the data product was developed primarily with the Arctic in mind and so most of the validation tests presented in the relevant papers (Huntemann et al. 2014, Paţilea et al. 2019), both ship-based and airborne data, have been done in Arctic. However, it is expected that the retrieval will work the same in the Antarctic. There are no fundamental differences for thin ice between the Arctic and Antarctic. Yes, flooding due to high snow load is more common in the Antarctic but it is also observed widespread in the Arctic (see e.g. publications about the N-ICE2015 campaign). Such conditions would hinder the retrieval in both hemispheres. However, for thin ice in the ice growth season such high snow loads are not typical. The retrieval was originally developed to assess sea ice thicknesses during freeze-up in near-100% sea ice concentrations. After some discussion prompted by concerns presented from all 3 reviewers, we decided for the sake of this manuscript to treat the SMOS and SMOS-SMAP SIT as apparent sea ice thickness. We think it's necessary to clarify that since we are using this retrieval for a region in which it is not optimized for, the exact values of sea ice thickness are not absolute. Despite the inaccuracy, this retrieval is based on the fact that at L band (1.4 GHz) the radiation is sensitive to SIT up to 50 cm (Kaleschke et al., 2010, 2012). Thus we believe, although not wholly accurate in terms of the exact values presented, it is still worth reporting wide-scale ice thinning near and above Maud Rise. However, computing ice volume from SIT maps that most likely have an inhomogeneous uncertainty distribution would likely result in erroneous results. As such, we want to keep this manuscript focused on the perceived signal and its areal as well as temporal distribution rather than taking the SIT retrieval at face value. We like to emphasize again that these disadvantages of the SIT retrieval are exactly the same for the Arctic and Antarctic. Also in the Arctic one can have little confidence in the absolute SIT values for SIC < 100% and under melting conditions. This is also independent of the particular retrieval used.

The ASI sea ice concentration (SIC) product is not meant to directly validate or correct the SIT product, but instead be used for comparison. This decision originates from previous analysis and comparison of passive microwave SIC algorithms to thin SIT (Ivanova et al. 2015, Heygster et al. 2014). Also from past studies (Huntemann et al. 2014, Paţilea et al. 2019), we know that both SIT retrievals are influenced by varying sea ice concentration, thus if we observed thinning on the same scale and continuously by the same amount for given sea ice concentrations, it would not be very promising. Despite this we have shown that during these thinning events and even polynya events, areas impacted by sea ice thinning are much larger than low sea ice concentration areas. As such, we believe it necessary to show this discrepancy as well report both data products over the span of the 2017 polynya and 2018 sea ice thickness anomaly. In addition, it helps to compare the two to study these events from different perspectives.

Specific Comments Reply:

- Line 1: This is minor point but there is some debate in the literature concerning what definition of a Weddell Sea Polynya (WSP). Some authors (e.g. Cheon and Gordon 2019 and Kurtakoti et al. 2020) argue that a WSP is strictly one that grows beyond Maud Rise and occupies much of the Weddell Sea, such as those that occurred in thelate 1970s. They distinguish these polynyas from the smaller Maud Rise Polynyas (MRP), which are largely confined to the vicinity of Maud Rise. By this definition, the 2017 event was not a WSP but a large MRP. While I think the distinction between MRP and WSP is rather superficial, the authors should make a deliberate decision with their naming convention. If they deem the 2017 event a WSP, I think it should follow that the 2016 event and other smaller events should be considered WSPs. Alternatively, the authors my sidestep this issue all together and just refer to these events generically as open-ocean or offshore polynyas in the Weddell Sea.

We went with the latter option as it was also suggested by the third reviewer. It is now referred to as the Weddell Sea polynya where “polynya” is lower case. Despite this change, we refer to it as “the Weddell Sea polynya” after clarifying the one we mean rather than specifying location at each instance.

- Lines 20-21: This relates to my previous point about the naming of polynyas. I don't think it's true that "no sizable opening" in the sea ice occurred between 1970s and 2016. There were substantial polynyas openings in the mid 1990s and early 2000s (see figure 5 of Campbell et al. 2019).

Statements will be refined to acknowledge the presence of openings in the intermediate years.

- Line 30: "...convection pushes up Warm Deep Water..." I think "pushes up" is an awkward word choice. I would replace with "mixes" or "entrains".

Changed to “entrains”.

Lines 30-35: See my major points at the beginning of this review. This section introduces the potential for storms to initiate polynyas but only cites Campbell et al. (2019). Several other studies need to be acknowledged here (e.g. McPhee et al. 1996, McPhee 1999, Goosse and Fichefet 2000, Wilson et al. 2019, and Francis et al. 2019). Francis et al. 2019 in particular gives a fairly detailed account of atmospheric conditions leading up to the opening of the 2017 polynya.

References will be added.

- Lines 39-50: In this discussion about why Maud Rise is so favorable for polynya formation, I would cite Martinson and Iannuzzi 1998 and Wilson et al. 2019. Both use in situ ocean data to demonstrate that the Maud Rise region has a unique combination of weak stratification and high sub-mixed layer heat content that primes the region for strong wintertime basal melting and deep convection.

References will be added.

- Lines 43-44: Unclear description. Is the intent here to convey that the seamount affects ocean heat fluxes over an area that is twice the footprint of the seamount itself?

Yes, that is correct. We will reformulate to clarify this part.

- Line 45: The core of the ACC, as defined by the polar front, is much further north than Maud Rise. This major current here is simply the southeastern limb of the Weddell Gyre.

Noted, the text you are referring to will be corrected accordingly.

- Line 73: Is there a simple explanation for why this 50 cm thickness limit exist? Is this limit affected by the presence of snow?

See explanation in the answer for (ii). We will add a sentence to clarify the limit at this location in the paper.

- Line 92: First sentence is unclear. Were these algorithms were derived from past studies that have focused estimating Arctic sea ice thickness?

Yes this is correct. Wording will be made clearer.

Line 97: I think these "subtle differences" between the two polar regions are important and should be described in more detail.- Lines 100-110: As I mentioned earlier, these relatively large adjustments associated
with lower than 100% SIC is rather surprising. I think more justification is needed here.

Comments from you and reviewer 3 were noted, and the whole Data/Methods section will be revised to convey our change in approach from SIT to apparent SIT which should make everything clearer hopefully.

- Lines 112-120: Is SIC derived from the SMOS or SMAP sensors?

No it’s from the AMSR2 satellite onboard JAXA's GCOM-W1 spacecraft - further clarified in the manuscript.

- Lines 147-148: It might help to label the subplots that referenced here.

For the figure we're referring to here, we decided to split it into 1 figure that simply shows where the location of interest is and another that shows the 2018 maps as you also suggest in your comments about figures. Thus labelling should not be necessary.

- Line 152: After "satellite imagery" add "and in situ ocean data".

The text you proposed has been added.

- Line 155: Here are elsewhere, the patches of low sea-ice thickness are referred to as "polynyas". Strictly-speaking a polynya describes ice-free conditions. Since the overlying ice did not completely melt, a polynya did not form. Another word or phrase is needed to describe these "polynya-like" events.

Sentence changed to: “August and September 2018, shown in the time series plots of Fig. 4, is the time period of interest for this research, where the area that featured a polynya the year prior shows a low SIT anomaly”.

- Line 163: See previous comment. "polynya thin ice" is a bit contradictory.

Sentence omitted as the point being made in it was self-explanatory.

- Lines 165-168: Campbell et al. (2019) and Francis (2019) both report these wind Anomalies.

References have been added.

- Line 183: I would replace "more plausible scenario" with something like "a more complete perspective". The 2D versus 3D views of sea ice cover are not contradictory. It remains true that no polynya appeared over Maud Rise in 2018. So in that sense, the shift from record breaking winter polynya in 2017 to no polynya in 2018 was rather abrupt. With this new SIT data, we now see that 2018 represented a waning of polynya favorable conditions rather than an abrupt end. Furthermore, nothing in this study discredits the hypothesis there may have been local freshening of the surface layers in this region.

Wording changed as suggested.

- Line 197: The discussion here is incomplete. While it is true that the loss of heat would destabilize the water column, the thinning of the sea ice implies melt, which strengthens the stratification. At these low temperatures, the stability of the water column is set by salinity. My suspicion is that this partial melting of the sea ice created a thin halocline that stabilized the water column and suppressed further exchange with the Warm Deep Water layer (Martinson 1990 and Wilson et al. 2019 describe this negative feedback in more detail). The suppression of vertical mixing effectively protected the sea ice from further basal melting, which caused no polynya to occur. I would further speculate that if a stronger storm had passed through the region, we would have seen another polynya reappearance in 2018.

The line indicated was referring to the 2017 polynya, not the 2018 anomaly. In any case, this entire section was re-written on account of reviewer 3 who proposed reading up on the oceanographic literature which identified the inaccuracies in the text.

- Line 198: It's not clear to me how the destablization isolates the water mass above Maud Rise. Please elaborate.
this region.

Rewritten, as mentioned above.

- Line 199: Up until this point, the discussion has been entirely focused on the area above Maud Rise. We have no information about the stability of the water column across the entire Antarctic sea ice zone.

Rewritten, as mentioned above.

- Line 203: This presence of this convection cell is speculative. This heat may have been brought up by strong wind-driven vertical mixing or Ekman divergence.

Rewritten, as mentioned above.

- Lines 210-212: This sentence is hard to follow. I would rephrase for clarity.

Rephrased.

- Line 214: Campbell et al. 2019 and Francis et al. (2019) discuss wind-driven ice divergence.

References have been added.

- Lines 220-221: I don't think by comparing wind conditions in 2017 and 2018, one can make a general conclusion about when winds can or cannot generate a polynya. This evidence is rather anecdotal.

Reworded appropriately. Now written as: “Through the comparison of our SIC data with ERA5 atmospheric data we can speculate what wind conditions are favourable for polynya formation.”

- Lines 237-238: Please cite the relevant studies here.

References have been added.

- Line 244: I would replace "anticipate" with "suggested by SIC data"

The change in wording has been implemented.

- Line 251-252: As I mentioned before, numerous studies have "rigorously" explored the idea that wind-driven mixing may drive substantial melting.

These studies were mentioned and referenced.

- Line 265-266: I very much agree with this statement. At some point, perhaps not at this
exact line, the authors should stress that the melting of sea ice produces a strong negative feedback that suppresses further entrainment of deep ocean heat. This effect discourages deep convection, which is required to sustain a polynya. A sequence of sufficiently strong storms may override that feedback by entraining enough WDW that completely melts the ice cover and eliminates the pycnocline.

Information on these processes will be included and mentioned in our Discussion section. Thank you for pointing them out to us.

- Lines 275-278: These uncertainties should be discussed in more detail in the methods section.

Yes, this will be addressed in the revised Data/Methods section.

- Line 290: I don't understand what is meant by "purely-open ocean polynya".

Clarification added. Changed to "purely ocean-driven".

Figures:

Figure 1: These plots are a bit counter-intuitive. At first glance, it would appear that upticks in thin sea ice represents new growth from open-ocean conditions, when in fact they represent thinning from thicker sea ice. I would suggest filling in the space between each line with a given color as well as the top region above the green line, which represents "thick ice". Also, why is 30cm instead 50 cm used as the threshold for thin/thick ice? One might also consider normalizing the area such that the lines represent the fractional area covered by sea ice below a certain thickness.

Excellent suggestion which made things clearer. The figures now follow this basic layout.

Figure 2: A few things here. I would make the zoomed out map of the Antarctic sea ice field a standalone figure and have that be the first figure of the paper. This would help orient the reader before they examine the line plots shown in the current Figure 1. By removing that zoomed out map, the remaining figure can then be arranged in the same format as Figure 3 to facilitate better comparison. Lastly, all these plots should have higher resolution.

. Resolution has been increased. 2018 side-by-side will also be added.

Figure 4: As in Figure 2, I think plots (b) and (c) would be better visualized if the area above and below these lines were filled with color. The time axis is also difficult read. I

would suggest labelling fewer tick marks (e.g. every 5 days) and making the labels horizontal.
Figure 5: See above.

Here again, the suggested changes really improved the output. Thank you for that.

References:

G. Heygster, M. Huntemann, N. Ivanova, R. Saldo and L. T. Pedersen, "Response of passive microwave sea ice concentration algorithms to thin ice," *2014 IEEE Geoscience and Remote Sensing Symposium*, 2014, pp. 3618-3621, doi: 10.1109/IGARSS.2014.6947266.

Ivanova, N., Pedersen, L. T., Tonboe, R. T., Kern, S., Heygster, G., Lavergne, T., Sørensen, A., Saldo, R., Dybkjær, G., Brucker, L., & Shokr, M. (2015). Inter-comparison and evaluation of sea ice algorithms: towards further identification of challenges and optimal approach using passive microwave observations. *The Cryosphere*, *9*(5), 1797–1817. https://doi.org/10.5194/tc-9-1797-2015

Huntemann, M., Heygster, G., Kaleschke, L., Krumpen, T., Mäkynen, M., and Drusch, M.: Empirical sea ice thickness retrieval during the freeze-up period from SMOS high incident angle observations, The Cryosphere, 8, 439–451, https://doi.org/10.5194/tc-8-439-2014, 2014.

Paţilea, C., Heygster, G., Huntemann, M., and Spreen, G.: Combined SMAP–SMOS thin sea ice thickness retrieval, The Cryosphere, 13, 675–691, https://doi.org/10.5194/tc-13-675-2019, 2019.

Kaleschke, L., Maaß, N., Haas, C., Hendricks, S., Heygster, G., and Tonboe, R. T.: A sea-ice thickness retrieval model for 1.4 GHz radiometry and application to airborne measurements over low salinity sea-ice, The Cryosphere, 4, 583–592, https://doi.org/10.5194/tc-4-583-2010, 2010

Kaleschke, L., Tian-Kunze, X., Maaß, N., Mäkynen, M., and Drusch, M. (2012), Sea ice thickness retrieval from SMOS brightness temperatures during the Arctic freeze-up period, *Geophys. Res. Lett.*, 39, L05501, doi:10.1029/2012GL050916.