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

General Reply (to Reviewer 1):

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.

- McPhee et al 1996, Goosse and Fichefet 2000, Wilson et al. 2019 and Francis et al. 2019 have been cited and the ANZFLUX field campaign as well as their works mentioned, respectively.
- Thanks to yours and reviewer 3’s comments, the wording has been corrected so as not to imply our results are unique and are now instead corroborating these past studies.

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.

- The Data and Methods section has been rewritten with a key difference being that we redefine the sea ice thickness (SIT) data as apparent SIT (ASIT) to convey the inaccuracies that come with applying SMOS-SMAP SIT analysis to a region like Maud Rise that is susceptible to low sea ice concentrations on a regular basis.
    - With this change, we aim to focus more on the distribution as well as recurrence of low SIT anomalies rather than the exact thinning observed at each pixel (which was the original aim of the manuscript).
- The limitations and uncertainties of the retrieval with regard to decreasing SIC and near-50 cm SIT is now covered and made clearer.
- The lack of sea ice volume calculations has been justified.

Specific Comments Reply (to Reviewer 1):

- 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).

> Sentence changed to "For the next 40 years the few polynya events are comparatively smaller (Campbell et al., 2019) and often in the form of a low sea ice concentration (SIC) halo around Maud Rise (Lindsay et al., 2004).".

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

> This sentence was omitted and ocean-driven polynya formation is now covered in detail in paragraph 1 of the introduction.

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.

> All indicated references were added in the 4th paragraph of the rewritten introduction section that discusses the impact on wind at Maud Rise in more detail.

- 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.

> Both references were added (line 41-42); this discussion is now the third paragraph of the introduction.

- 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. The sentence was changed to “Muench et al. (2001) go further to state that Maud Rise facilitates an upward transport of Warm Deep Water that affects a sea ice area that is roughly twice the size of the seamount” for clarity.

- 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.

ACC changed to the southeastern limb of the Weddell Gyre.

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

Explanation for the limit added in paragraph 3 of section 2.1. While the presence of snow is a factor, the main contributor for the cutoff is the penetration depth of the 1.4 GHz frequency used.

- Line 92: First sentence is unclear. Were these algorithms were derived from past studies that have focused estimating Arctic sea ice thickness?
    - Lines 121-123 now briefly summarizes the development process of the precursor SMOS algorithm which was derived through comparison with a Cumulative Freezing Degree Days (CFDD) model as opposed to other SIT products.
    - The line in question was left as is, as the paragraph it begins is simply meant to briefly discuss the improvement of the retrieval by also considering the SMAP satellite.

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.

- Our switch from SIT to ASIT is meant to justify the use of this data product for the region despite the differences mentioned (elaborated on in the re-written section 2.1).
    - Due to the lack of validation studies that quantify the retrieval uncertainty associated with sea ice differences in the Arctic and the Antarctic, we thought it necessary to make this change.

- 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 (now mentioned in lines 136-138).

- 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 (now Fig. 1 as you suggested) and another that shows the 2018 maps (now Fig. 4).

- 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.

All your speculations are reflected in the re-written discussion section.

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

The sentence had flaws and upon reading more literature on the topic, was omitted altogether. Thanks in part to you and reviewer 3.

- 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.

The sentence was also omitted for the same reasons.

- 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.

The concept you mention is now reflected in the re-written discussion section.

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

This sentence has been omitted in the revision process.

- 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.

Both your points were mentioned in the revised conclusion section.

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

- The sentence has been moved to data and methods (section 2.1), however no further elaboration on the degree of uncertainty was given.
    - This is because of the impossibility to definitively assess the spatial and temporal distribution of flooding via remote sensing.
        - This limitation is now mentioned in section 2.1 as well.

- 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 that you suggested.

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.

The resolution was increased. 2018 side-by-side has also been added (now Fig. 4).

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 too, the figures have been changed as you suggested.

General Reply (to Reviewer 2):

Based on the definition of this type of polynya, open water is obvious indicator of the polynya. Thin ice is an indirect indicator that shows open happened here but now thin ice and will soon become thick ice if the warm water upward does not appear again in a later date. In my opinion, the open water is better mapped by the ASI ice concentration map (6.5km resolution) as indicated in the paper and prior studies. The new thin ice thickness product is 45km and is not a good indicator of the small opening; also there is publication about thin ice thickness retrieval from passive microwave remote sensing such as the AMSE-E/2, that could be up to 6.5km in spatial resolution (see paper Dai et al., 2020, Remote Sens. 2020, 12(9), 1484; https://doi.org/10.3390/rs12091484). However, this paper did not mention this method in their paper at all, indicating some lack in literature review. To confirm and validate their thin ice method for mapping polynya, Sentinel-1 SAR image is a much better approach than the ASI concentration since the Sentinel-1 has much higher spatial resolution (also in Dai et al., 2020 paper and other papers).

- We have opted not to cite papers: Nihashi et al. 2017 and Dai et al. 2020

    - We believe due to the lack of studies done on the method developed by Nihashi et al. 2017, the mentioning of this paper would lead to more questions rather than answers that will not benefit the scientific discussion at hand
    - A lot of observed thinning was in the 30-50 cm range, which the algorithm from Nihashi et al. 2017 cannot pick up on.
- We have opted to stay with ASI SIC and MODIS for comparison with our ASIT record as using Sentinel-1 SAR images would mostly serve as a visual guide (what we already partly achieve with MODIS)
    - We believe the AMSR-E/2 ASI SIC product can more reliably report on the ice concentration in the area of interest

Specific Comments Reply (to Reviewer 2):

In the paper, authors made it clear that their SMOS-SMAP retrieval algorithm assumes ~100% SIC, while there is low SIC with polynya. This causes a concern on their results.For example, in their text line 108-110, “Thin ice thickness is … a combined ice area and thickness anomaly and not be used to calculate... ice volume…”.

- Data and Methods (section 2.1) has been rewritten to specify that we care about apparent SIT (ASIT) to observe the spatial and temporal distribution of thinning rather than the degree of thinning itself.

This paper claimed that it is the first time to confirm that wind is a major factor for the Weddell Sea polynya, although I am not sure if they have enough data to confirm this finding from these polynya events 4o years ago. Otherwise, are you so confident that the results from these two years of data can apply to other times?

- The wording that implies we were the first to discover this has been correctly adjusted.
- We now explicitly state that we simply corroborate past studies with the short time period that we ourselves analysed

they conclude various different factors must occur simultaneously for the polynya to occur. But this statement is not an approved statement. the paper writing needs to improve, I have listed a few in the details below, but many can be found throughout the paper. One big comment is the section 4 (conclusions). Most of the content in this section should be in the discussion not in the conclusions

The conclusions section is written in a way that reflects what we conclude based on the discussion section and the plots and maps shown in the results section. As such we opted not to move over anything so as not to interrupt the flow we established when we first wrote up the manuscript.

L2, “fully opened again on …2107”, but figure 1 and text shows 2016 opened.

L3, “lasted until melt” is not clear and confused. Maybe change to “lasted until the summer melting season”? “80 days, 2017,” should be “80 days in 2017.”, right? are you sure it reached early December before all surrounding ice melted?

The indicated passage has been rewritten in light of reviewer comments and is now as follows: “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.”

L4, “actually was not the…”, what is the subject of this sentence? You missed it.

The subject is the year 2017, the full sentence is now: “2017, however, actually was not the only year the imprint of the polynya could be identified.”

L59-61, I have question for this purpose of the study: “we aim to ….”, why you want to using the thin ice thickness which is not already existing and will be much coarse resolution as compared with the exiting ASI ice concentration data. AMSR-E/2 can also be used to derive thin ice thickness. Is this your thickness compatible with the AMSR-E/2 Derived?

- The SMOS-SMAP SIT product was developed in 2019 (Paţilea et al. 2019) and its precursor SMOS SIT retrieval was developed in 2014 (Huntemann et al. 2014), so at the time of writing this manuscript, both data products were already in existence and made available.
- To our knowledge there was no AMSR-E/2 sea ice thickness product that was available at the time of writing the manuscript.

L85: a root mean square difference (RMSD)

Now written out.

L92, “growing sea ice”, do you mean “sea ice growing season”?

The indicated sentence now reads: “Both the SMOS-SMAP and SMOS are empirical retrievals that were initially developed for monitoring the sea ice thickness of growing sea ice in the Arctic during freeze-up through comparison with a Cumulative Freezing Degree Days (CFDD) model and thereafter calibration and validation using observations (Huntemann et al., 2014)”

L110-111, I am not sure how long this thin ice would last once the upward of warm deep water stops or weaken, since most of the time the Weddell Sea polynya is an open water area as indicated (for 80 days in 2017). Once the upward of warm deep water stops, thin ice would form and would thicker, also thicker ice from surrounding would come to fill the open and thin ice area soon as I can imagine.

The indicated sentence has been omitted.

L141: Can you explain why 2017 and 2018 are chosen?

Reasons behind the choice were clarified. Sentence was changed to: “For a detailed analysis of the Weddell Sea polynya, 2017, the year in which the largest Weddell Sea polynya of this century occurred, and 2018, the year that followed which as will be shown exhibits anomalous thin ice behaviour, have been chosen.”

L143-144: Can you clarify the strength of SMOS-SMAP SIT compared to the SIC datasets?

Indicated sentence changed to: “Here the advantage of the SMOS-SMAP ice thickness retrieval shows its strength by detecting anomalous sea ice behaviour where traditional sea ice concentration datasets cannot”

L149: Can you mention this spatial resolution of ASI SIC in section 2.2?

The spatial resolution has been included at the end of the paragraph contained within section 2.2.

L169, “….area that is classified as open water”. I have question. this means it was 0% open water before Sept 13? if this is the case, then 100% ice covered, then ice thickness should be higher but why it was actually lower compared with after Sept 13? this is not possible. am i wrong?

On September 12 there was a bit of open water area left (below 80% class in Fig. 4b) while the entire area is ice covered on Sep 13. Technically this means that this area could be refrozen, i.e., it should be detected in the thin ice class (<10cm). However, because of the difference in Resolution of the two products this is not an enforced causality. The reduction of open water area in the footprint of SMOS/SMAP could just increase the retrieved ice thickness. The latter is consistent with what is shown in Fig. 4c as reduction of low SIT area across all SIT classes.

L170, 0% open water should come with high SIC and SIT, right?

Yes, this is correct, see also the previous answer.

L175: There is no section 3.2. Please check it. Should it be “4. Discussion”?

You are correct. This had been corrected.

L180: Can you add any references here?

Appropriate references have been added to the sentence in question.

L191: Can you present any statistical parameters, such as correlation coefficient (R)?

- No, no correlation studies were done nor were they the focus of the manuscript.
- In principle, correlation studies between SIC and SIT would only serve to distinguish the two datasets which we already sufficiently show in the maps and time series.
- Wording that implied that correlation studies were conducted has been removed

L193-194: I guess the higher resolution of ASI SIC can affect this result. In Figure 3 and Figure 6, it seems that ASI SIC (6.25 km) has a much finer spatial resolution than SMOS SIT (45 km?). If so, the ASI SIC data should underestimate the thin-ice (or open water) area compared to the SMOS SIT data. Maybe you need to discuss the effect of different spatial resolution.

Since ASI retrieval is estimating open water as a percentage on a sub-footprint scale, as is SMOS and SMAP-SMOS but with SIT, we do not think any sizeable under(or over)-estimation will occur in either data product.

L201-203, sentence “we see… freeze up”, please break up…

The indicated sentence has been omitted and large parts of the section completely rewritten.

L213, “east and southeast directions”, but the figure 4 does not show these directions.

Figure 7 referenced for the direction specifications.

L216-219, Figure 7: Can you mark the extent of sea ice anomaly area in Figure 7?

Thank you for the suggestion. While the exact shape of the polynya, and the anomaly even more so, is difficult to establish. A reference oval has been included in the wind charts to guide the viewer.

L226-227, “therefore also…”, please cite papers here since it is your finding.

The indicated sentence has been changed to: “Thereafter also turbulent mixing of warm salty water plays an increasing role (Campbell et al., 2019).”

L 237-238: I just wonder if there is any possibility of sea ice advection (e.g. drift of thin sea ice from other regions?). By seeing the time-series video of SIT, you may be able to confirm if those thin ice events are all really “polynya-type” events or they are advection of thin ice from surrounding area.

More generally, literature on the topic attributes this thinning to melting from below. This is partly confirmed for the 2017 polynya (e.g. Campbell et al. 2019) with in-situ data and the situation can be assumed to be similar for 2018 since the region is prone to such events (Wilson et al. 2019, Holland 2001). The maps chosen were those that best depicted the initial expansion of 2017 as well the wide-scale thinning in 2018, however longer time periods were studied and the videos you are referring to were analysed. As such we can confirm that this is not simply the advected thin ice.

L268-269, the last sentence “Moreover, it is the combination….”. this sentence is suspicious, since your paper did not approve it.

Here we try to corroborate past studies on the topic rather than claim it as our finding. The inclusion of relevant papers and the buildup to that sentence, as well as the better wording throughout the manuscript, hopefully makes that clear now.

L281, “…low SIC (most likely minor lead openings) is recorded”. I really hope the paper use the Sentinel-1 SAR data to validate or confirm…

In the end, we decided this is beyond the scope of this manuscript and we’ll stick with MODIS blue marble imaging for high-resolution comparisons. We do not mention Sentinel-1 SAR specifically in the outlook portion of the Conclusions section, but do encourage additional validation and thereby improvement of our SIT retrieval.

Figure 1: Can you briefly explain how to define/distinguish “polynya events” and “ice thinning anomalies”?

Already in the introduction we now have: “...thin ice area anomalies, i.e., thinning of ice on the same scale as the polynya that is subject to similar underlying causes.”

Figure 3. what are the resolutions for them? it is really not easy to match the two sets.

The SMOS/SMAP product is oversampled in a resolution of 12.5km while the size of footprint is about 45km on average (hence the soft contours). The ASI resolution is about 5km and it is resampled into a 6.25km grid.

Figure 4 and Figure 5: I am just curious why you mention 2017 (Figure 5) first, and then 2018 (Figure 4). Would it better to mention 2017 first prior to 2018? In Discussion, you describe 2017 first and then 2018, so it is somehow confusing to read the text and figure together. And same for Figure 7 (2018) and Figure 8 (2017). Also Figure 4b, if 0%

SIC for open water, should be 100% sea ice, but figure 4c shows 0% ice from August 11-Sept 4, please explain?

- The flow of the text has been switched to consistently be 2017 discussed first and then 2018. Thank you for that insight; it has added clarity to the structure of the manuscript.
- The figure you are referring to (now Fig. 6) shows the area of the different sea ice thickness classes where the thick ice class (>50 cm) is not shown in the plot. This means that the entire area was covered by thick ice beyond the thickness sensitivity of the SMOS/SMAP retrieval.

Figure 6, really the SIC and SIT do not match much, except the 2016 and 2017.

No and they are not meant to, that is partly what this manuscript aims to show that areas of thin ice, in this setting where a lot of the melting is from below, need not be low in terms of sea ice concentration.

Figure 8: Same to Figure 7, can you mark the extent of sea ice anomaly area in Figure 8?

A reference oval indicating the location of the polynya has been included.

General Reply (to Reviewer 3):

Title, Page 1: I would suggest considering a more descriptive title that reflects your paper's broader scope beyond the 2017 polynya. As an example, something like: "Sea ice thinning at Maud Rise identified in SMOS-SMAP record". In any case, please standardize the capitalization of the title that you choose, e.g., for the existing title: "Weddell Sea Polynya analysis using SMOS-SMAP sea ice thickness retrieval".

We went with the latter option and standardized the capitalization for our existing title.

Introduction, Pages 1-3: I am concerned that the Introduction section's summary of Weddell polynya formation begins and ends with papers published in 2019 and 2020. As currently written, it neglects the four-decade-long body of literature on the Weddell polynya phenomenon, particularly on the polynya's relationship with preconditioning, stratification, convection, and eddies at Maud Rise. I recommend, at a minimum, consulting Gordon (1978), Martinson et al. (1981), Motoi et al. (1987), Comiso and Gordon (1987), Gordon and Huber (1990), Martinson (1990), Holland (2001), and de Steur et al. (2007) – none of which are cited in this manuscript – and expanding the Introduction by briefly summarizing at least a few of these foundational papers. Please also take some time to think about how these previous studies relate to your findings and cite some of them throughout your Discussion and Conclusions sections where appropriate. I make this request as I feel it is critical to reference and build on past work, especially when new science challenges long-held paradigms, as yours does. Furthermore, in addition to being incomplete, I found the Introduction difficult to follow, as it skips around between unrelated points. It would help to organize it by common themes, rather than just summarizing one paper after another. I recommend reorganizing the entire Introduction. A logical order to address topics (~1 paragraph each) might be:

1. Past observed Weddell polynyas
2. How Weddell polynyas are formed by destratification and maintained by convection
3. Mean-state factors that favor/precondition polynyas at Maud Rise (low stratification, high heat fluxes,
4. Taylor column, eddies, etc.)
5. Role of interannual variability (SAM) in preconditioning polynyas, including in 2016-2017
6. Role of subseasonal synoptic variability in triggering polynyas (storms, possibly eddies)

- We admit to the lack of literature review that you pointed out and have spent time reviewing the suggested literature.
- Previously erroneous speculations presented in the manuscript have been rewritten and improved (namely the whole introduction section, the 2017 and 2018 paragraphs in the discussion section and large portions of the conclusions).
- The introduction has been reorganized to a large extent as you had suggested.

Introduction, Page 2, Lines 28-29: Cheon and Gordon (2019) are not the first to attribute Weddell polynyas to weak stratification, but one would not be aware from reading this passage. This explanation goes back to Martinson et al. (1981) and the other papers I've mentioned above.

The paragraph within the revised introduction section that addresses that topic (point 2 in your laid out plan for the logical order of the introduction section), now cites all the following works: Martinson et al., 1981; de Steur et al., 2007; Wilson et al., 2019; Cheon and Gordon, 2019.

Introduction, Page 2, Lines 30-38: Please follow the suggestions of Reviewer #1 regarding this passage. Others besides Cheon and Gordon (2019) and Campbell et al. (2019) have made the case for storms being important – or even critical – for polynya formation near Maud Rise, and those studies should be acknowledged. Additionally, the phrasing “admit to” feels overly disparaging towards Cheon and Gordon (2019), who discuss “atmospheric influences” in considerable detail. Please change this phrasing; you could be more specific and say that their study discusses large-scale, climate-related atmospheric effects, but not synoptic scale meteorological variability.

- The paragraph within the revised introduction section that addresses that topic (point 6 in your laid out plan for the logical order of the introduction section), now cites all the following works: e.g., McPhee et al., 1996; Goosse and Fichefet, 2000; Francis et al., 2019; Campbell et al., 2019; Wilson et al., 2019; Heuzé et al., 2021.
- The sentence you are referring to that previously cited Cheon and Gordon (2019) has been removed and no such disparaging terminology is used.

Results, Page 8, Lines 165-167: This was a major finding of both Francis et al. (2019) [see their Fig. 5] and Campbell et al. (2019) [see their Extended Data Fig. 4], as Reviewer #1 mentions. Those papers should be cited.

Both the studies you mention are now briefly summarized and cited in the revised introduction section.

Conclusions, Page 14, Lines 251-253: As Reviewer #1 points out and as my comments throughout have indicated, previous studies have, in fact, put forth “rigorous” explanations regarding the influence of atmospheric perturbations (e.g., storms) on ice melt and polynya formation at Maud Rise. They should be cited here. Also, Cheon and Gordon (2019) is not the only study that has discussed the role of large-scale negative wind stress curl: Campbell et al. (2019) and Cheon et al. (2014, 2015, 2018) have also discussed this.

- researchers that have studied the influence of atmospheric perturbations on ice melt and polynya formation at Maud Rise have been cited.
- We did not mean to claim that our findings are the first in that respect, but admit to how the phrasing in the manuscript could have implied it - said phrasing has now been corrected.

Introduction, Page 2, Lines 37-38: This sentence is almost entirely copied verbatim from two sentences on p. 320 of Campbell et al. (2019), to the extent that this could be considered plagiarism. You must rephrase this. You’ve also combined two unrelated ideas (divergence of ice preventing stabilization by ice melt, and turbulent mixing leading to heat/salt entrainment) from their study and so the sentence is not coherent as written.

The sentence now reads: “Ice divergence due to strong winds enables rapid ice production and brine rejection preventing stabilization from ice melt as wind-driven turbulent mixing entrains warm and saline water into the surface mixed layer (Campbell et al., 2019)."

Data/Methods, Page 5, Lines 131-133: This sentence is copied nearly verbatim from Campbell et al. (2019). This is not permissible and could be perceived as plagiarism, like the other instance I mention above. I understand the wish to provide these specific details, but the sentence must be at least rephrased.

The sentence now reads: "Campbell et al. (2019) report that there was sufficient agreement between mean sea level pressure (MSLP) data obtained from the SANAE-AWS weather station and the nearest ERA-Interim grid cell (1979-) for ERA-I to be used in gathering signs of storm activity as it skillfully represented MSLP variability near Maud Rise."

Introduction, Page 2, Line 39: Saying "thus far all discussed preconditioning is... [not] exclusive to... the region of interest" is not accurate: (1) Preconditioning mechanisms have barely been discussed in the preceding paragraphs. (2) All of the preconditioning mechanisms are, in fact, exclusive to the region of interest. The reason that positive SAM fluctuations and an enhanced Weddell gyre circulation result in preconditioning is that they increase the doming of isopycnals in the center of the Weddell gyre, where Maud Rise is located, thus uplifting warm and salty Weddell Deep Water.

The paragraph within the revised introduction section that addresses that topic (point 3 in your laid out plan for the logical order of the introduction section), now makes no such claims.

Data/Methods, Page 4, Lines 92-99 (also Lines 275-277): The lack of published calibration and validation of the SMOS and SMOS-SMAP thin ice thickness retrievals in the Antarctic is hugely concerning to me. Your paper's analysis and conclusions rest on a highly uncertain empirical satellite retrieval for which no published validation has been conducted for sea ice in the Southern Hemisphere. I request that several actions be taken to mitigate this issue:

1. Please be explicit in Line 92 that the retrieval is empirical and that it was developed through comparison with a simple Cumulative Freezing Degree Days (CFDD) model of Arctic sea ice growth, then calibrated and validated using other Arctic sea ice estimates (Huntemann et al. 2014).

    This has been briefly summarized in the following sentence from section 2.1: "Both the SMOS-SMAP and SMOS are empirical retrievals that were initially developed for monitoring the sea ice thickness of growing sea ice in the Arctic during freeze-up through comparison with a Cumulative Freezing Degree Days (CFDD) model and thereafter calibration and validation using observations (Huntemann et al., 2014)."

2. Make clear throughout Section 2.1 that the lack of Antarctic validation is a limitation and discuss the reasons why this is the case. For example, discuss the sensitivity of the SMOS/SMOS-SMAP retrieval to overlying snow cover, sea ice salinity, and other ice/snow parameters, and mention the degree to which these parameters differ between the Arctic and Antarctic. Additionally, the retrieval was trained on a CFDD model that applies well to the Arctic but probably not the Antarctic, where ocean heat fluxes are stronger and significantly influence sea ice growth in weakly-stratified regions such as near Maud Rise (see, e.g., Wilson et al. 2019). Please discuss the potential impact of different rates of ice growth in these regions.
    - We have made it clear that we do not claim our SIT algorithm retrieves exact values of thickness.
        - We now instead focus on the temporal and spatial distribution of thinning and refer to our product, for this study, as apparent SIT (ASIT)
    - The effect these differences can have on the product are purely speculative due to the lack of validation studies done in the Antarctic; we can only infer that the uncertainty goes up but do not know the degree to which it does so - as a result we opt to mention these sources of uncertainty but in view of our shift in focus as well as lack of validation choose not to discuss them individually

3. Be explicit that the "minor evaluation tests" mentioned in the Antarctic (Lines 92-96) are unpublished. Mention what region the EM-bird validation tests occurred, over what time period(s), and over what ranges of sea ice thickness. Give more detail about the degree of agreement between the EM-bird and SMOSSMAP data at different sea ice thicknesses within 0-50 cm. Please consider including a figure showing this validation exercise. Importantly, this will help provide a reference for the community as others publish studies using the SMOS-SMAP data for Antarctic research, which is inevitable given that you have publicly released the Antarctic data and it is increasingly being used.

    This manuscript, first and foremost, was meant to be an application of the SMOS and SMOS-SMAP sea ice thickness (SIT) retrieval to the Weddell Sea polynya over the available time frame. As you have already informed yourself from this manuscript and more so from Huntemann et al. 2014, Paţilea et al. 2019 and Kaleschke et al. 2012, this goal is hindered by the fact that we are applying a method that was validated and developed for high ice concentration locations undergoing freeze-up, to a dynamic area that is subject to cycles of melt and refreeze as well as exposing large areas of open water. Due to concerns expressed by all reviewers, we have now shifted the focus from taking the SIT retrieval at face value, and analyzed the retrieved signal as ASIT. Unfortunately, the inclusion of the EM-bird validation will not aid this research as it was done in areas with thicker ice and was only processed with a preliminary calibration. What is more, most of the evaluated sea ice thicknesses are above 50 cm, which due to the high levels of uncertainty at those thicknesses, were cut out from the finalized and published dataset. For these reasons, we have opted not to include the validation but nevertheless mention it.

4. Add a straightforward figure to this paper, perhaps in the Appendix, showing the uncertainty of the SMOSSMAP retrieval at sea ice thicknesses spanning 0-50 cm, perhaps modeled after Figure 9a in Paţilea et al. (2019). However, note that their Figure 9a (which already shows substantial error of ~30 cm for a thickness of 50 cm) is based on a SIC uncertainty of 5%. Given the 'halo' of low SIC (85-90%) known to be present around Maud Rise (Lindsay et al. 2004), the deviation of SIC from 100% is much larger than 5% in this region. Please account for this larger SIC uncertainty when computing SMOS-SMAP uncertainties for this figure. This will help to address the concerns of both Reviewer #1 and myself.

   We decided low-SIC based SIT uncertainty derivation to be unnecessary in view of our decision to only focus on ASIT. The uncertainties are undoubtedly high due to reasons mentioned above, as such the exact values of sea ice thickness are likely not accurate, especially near the low SIC halo (Lindsay et al. 2004). Nevertheless, it has been shown that at L band (1.4 GHz) the radiation is sensitive to SIT up to 50 cm (Kaleschke et al., 2010, 2012). By comparing to SIC data, we show that the used SIT retrieval isn't simply retrieving low SIT values where the halo is; rather the SIT area is generally larger and with less steep gradients than where the polynya or sea ice anomaly are located in 2017 and 2018, respectively. We also show that the two datasets evolve differently through time. Now as we shift our approach to simply depicting the sea ice thinning area through time rather than trying to assign exact values to said thinning, we believe any further uncertainty analysis is not needed. In conclusion, the Data/Methods section has been rewritten and revised in accordance with the shift from SIT to ASIT and as such does not include any study that quantifies related uncertainties.
5. Clarify in Lines 105-107 whether the SMOS-SMAP SIT data you show in this study are biased high or low at SICs less than 100%. The language you use (e.g., "retrieved sea ice thickness," "50 cm... is just 28 cm") is confusing.

This has now been clarified in the 2nd paragraph of section 2.1.

6. Please do not frame the three recent papers that have used SMOS SIT estimates to answer scientific questions in the Antarctic as mitigating the uncertainties in this retrieval (as you imply in Lines 97-99). SMOS SIT data were not the focus of any of those papers, which all referenced a wider variety of data sources and thus were less dependent on the accuracy of the SMOS retrieval.

   These papers are no longer mentioned.

Discussion, Pages 10-11, Lines 195-205: As Reviewer #1 points out, these few sentences about the role of the ocean contain incoherent reasoning and baseless speculation. My best advice would be to read and digest more of the oceanographic literature on Weddell polynyas – which, as I mention in previous comments, has almost entirely been neglected in the references of this manuscript – and then to completely reconsider the relevance of ocean processes to the data presented in this study. I will emphasize two key ideas to consider, though I would encourage the authors to build on these by assembling their own reasoning and references:

1. The sea ice thinning observed to precede the 2017 polynya is not necessarily (and, in my judgment, is almost certainly not) associated with ocean "destabilization" or deep convection. Note that sea ice growth in the Antarctic is (approximately) governed by the balance of ocean heat input and atmospheric heat extraction. If ocean heat fluxes become greater, sea ice will cease to grow and may start to melt. A variety of factors can influence ocean heat fluxes:
   - Turbulent mixing due to ice-ocean shear (which itself is greater at high wind speeds) can deepen the mixed layer and entrain warm pycnocline waters.
   - Brine rejection from ice growth densifies and deepens the mixed layer, also resulting in
   - entrainment of warm pycnocline waters. In contrast, freshwater input from ice melt can rapidly shoal the mixed layer, limiting heat fluxes from warm waters below.
   - Upwelling due to large-scale cyclonic winds (Ekman upwelling) or smaller-scale eddies (which may produce "doming" of isopycnals) can uplift warm pycnocline waters into the surface mixed layer.

   Without a basic understanding of these three processes, I believe it is not responsible to speculate on the causes of the thin sea ice anomalies that you observe. To better understand these processes, I would recommend carefully reading the very relevant study by Wilson et al. (2019). Note that atmospheric variability may also play a role in modifying the energy balance of sea ice; for example, see the study on the 2017 polynya published in Science Advances by Francis et al. (2020).

2. To explain the time evolution and interannual variability of sea ice thickness at Maud Rise, it is important to consider the role of ocean stratification and storms. For example, Campbell et al. (2019) show reduced upper-ocean stratification at Maud Rise in 2016 and 2017 due to a saltier surface layer. Lower stratification means lower resistance to turbulent mixing-driven or brine rejection-driven entrainment of warm pycnocline waters. This could easily explain the thin sea ice anomalies observed preceding the 2017 polynya (more entrainment = larger heat fluxes = thinner ice). However, I hope that you can go further and think about why thin sea ice anomalies are also seen in other years, as well as the sub-seasonal time evolution of these anomalies and their relation to storm perturbations. As McPhee et al. (1996) discovered as the sea ice melted beneath their ice camp during the ANZFLUX experiment, passing storms can strongly affect both the ocean and sea ice over Maud Rise.

    The Discussion as well as Conclusions sections have been, for the most part, rewritten to reflect the above-mentioned points after reading through all the suggested literature as well as re-reading through manuscripts that were cited initially.

Discussion, Page 11, Lines 215-216: No, low SIC and strong winds do not directly cause upwelling of warm water. Upwelling is a distinct process from mixing or entrainment. I believe what you are referring to is either turbulent mixing of warm pycnocline water related to high wind speeds, or entrainment of those same warm pycnocline waters related to densification of the mixed layer from cooling or brine rejection. Please see my previous comment on this topic.

This has been corrected and upwelling is no longer erroneously mentioned in the text. This particular sentence has been omitted, and the containing paragraph has been rewritten.

Discussion, Page 11, Lines 226-227: Please cite Cheon and Gordon (2019) and Campbell et al. (2019) for this finding, which is not shown in your study. Change “upwelling” to “upwelling and/or mixing”; note my comments above regarding this distinction.

References have been added and the text updated according to your request.

Conclusions, Page 15, Lines 257-258: I disagree wholeheartedly. I don’t see how ocean heat loss during the 2016- 2017 polynyas would preclude the possibility of anomalously large ocean heat fluxes (rather than atmospheric forcing) producing thinner sea ice in

2018. See my comments above about the factors that control ocean heat fluxes, such as upper ocean stratification. Additionally, you do not assess wind speeds in most years of your 2010-2020 record, and so you have not demonstrated that wind speeds preceding/during the 2016-2018 polynyas or ice thinning events were particularly anomalous. Intense storms pass by Maud Rise every year; you have not shown that the atmospheric perturbations in 2016-2018 were more severe than in other years.

As with the Discussion section, the Conclusions will be re-written, this time with the knowledge gathered from all the suggested literature from you. We now see the error in our past speculative discussion from which we have drawn our conclusions, and we will correct them such that they are in line with past research on this topic. Similarly, all cases where we imply that we can generalize our results for all polynya/ice-thinning cases have been deleted or reworded as necessary.

Specific Comments Reply (to Reviewer 3):

Abstract, Page 1, Line 2: I'd echo the point made by Reviewer #1 regarding the somewhat controversial nomenclature of "Weddell Sea Polynyas". As they mention, some recent literature makes a distinction between "Maud Rise polynyas" and "Weddell Sea polynyas" (e.g., Kurtakoti et al. 2018, 2021) despite providing no objective quantitative or mechanistic criteria (e.g., size or geographic thresholds, multi-year persistence, distinct formation mechanisms, etc.) to definitively sort a polynya into one category or the other. On the other hand, your use of "Weddell Sea Polynya" as a catch-all designation raises the question of how large or persistent an opening must be to merit this formal, capitalized title. The 2017 polynya reached ~50,000 km2 in size. Could a much smaller opening only 500 km2 (22x22 km) also be considered a "Weddell Sea Polynya"? Many might disagree. How about one that is 5,000 km2? These questions are not hypothetical, as we know this phenomenon occurs over a broad spectrum of size and duration (see, e.g., Campbell et al. 2019; Heuzé et al. 2021). If you want to avoid addressing this issue, one option might be to change "Polynya" to a lowercase "polynya" throughout your manuscript and mention (perhaps in Lines 22-23) that "Weddell Sea polynya" simply refers to any sea ice opening near Maud Rise.

The nomenclature has been changed as you suggested i.e. all instances of Weddell Sea Polynya now have a lowercase polynya. Despite this, we still refer to it as the Weddell Sea polynya, as specifying it as a Weddell Sea polynya near Maud Rise each time seems redundant.

Abstract, Page 1, Lines 2-3: There are a few problems with this sentence:

- It is inaccurate to state that the WSP has been absent for 40 years. This also undermines your paper's argument that a moderate thinning in non-polynya years is notable. As Reviewer #2 notes, other smaller polynyas have appeared over Maud Rise, e.g., in 1980, 1994, 2005, 2016, and arguably in a significant fraction of other years (see Comiso and Gordon 1987; Holland 2001; Muench et al. 2001; Venegas and Drinkwater 2001; de Steur et al. 2007; Campbell et al. 2019; Heuzé et al. 2021). Please consider acknowledging this, e.g., by stating: "After 40 years of intermittent, smaller openings, a larger, longer lasting polynya appeared..."

    Phrasing changed as suggested.
- It is not clear how to precisely and objectively define when the 2017 polynya first appeared or what "fully opened" means. Campbell et al. (2019), for example, trace its origin to two openings that were actually first seen on September 3 and coalesced and grew in size from September 13-18 (see their Extended Data Fig. 8). You could say: "...appeared in early September, 2017..."

    Noted. This was also pointed out by other reviewers; will be clarified.
- To be more specific than "melt," use "spring ice melt season" or similar. As Reviewer #2's comment indicates, a polynya is already open and so it cannot "melt."

    Phrasing changed as requested.
- Consistent with my earlier comment, change "a total of 80 days" to "approximately 80 days."

    Phrasing changed as requested.
- In summary, here's what I would suggest: "After 40 years of intermittent, smaller openings, a larger, longer-lasting polynya appeared in early September, 2017, and remained open for approximately 80 days until spring ice melt."

    Phrasing changed as suggested.

Abstract, Page 1, Lines 8-9: This phrasing ("we present the strong impact storm activity has on sea ice") ignores that Campbell et al. (2019) and Francis et al. (2019) have already made a strong case for storm activity impacting the evolution of the 2016 and 2017 Maud Rise polynyas using similar or identical atmospheric reanalysis data sets, as Reviewer #1 also mentions. Consider changing this to: "... we corroborate previous findings on the strong impact that storm activity can have on sea ice at Maud Rise" (note that this is also avoids implying that your results can be generalized to other sea ice-covered regions, where the impact of storms may be quite different).

Phrasing changed as suggested.

Abstract, Page 1, Lines 9-10: First, it is unclear what you mean by "direct atmospheric forcing." Second, the grammar of the phrase set off by commas ("... in addition to oceanographic effects”) is not correct. This should be changed to something like, “... help consolidate the theory that the evolution of Weddell Sea Polynyas is controlled by atmospheric as well as oceanographic variability” [or ‘forcing’ or ‘effects’, whichever is most appropriate].

Phrasing changed as suggested.

Introduction, Page 1, Line 20: Cheon and Gordon (2019) is not the appropriate citation for the 1970s polynya. Please cite Carsey (1980).

Correct citation used as indicated.

Introduction, Page 1, Line 20: See my comment above on Lines 2-3 and the similar note from Reviewer #1. Polynyas have appeared in many years since the 1970s, and there is a substantial amount of literature discussing their past occurrence that is being neglected here.

Sentence changed to: “ For the next 40 years the few polynya events are comparatively smaller (Campbell et al., 2019) and often in the form of a low sea ice concentration (SIC) halo around Maud Rise (Lindsay et al., 2004).”

Introduction, Page 1, Line 22: “Anything comparable” feels very arbitrary. The 2017 polynya was not much larger than the 2016 polynya, which itself was not much larger than the 1994 polynya, which was not much larger than the 2005 polynya, and so on (see Fig. 5 in Campbell et al. 2019). These events exist on a continuum of size and duration, most likely stemming from similar physical processes, and so arbitrary cutoffs make little sense. It would be more accurate to just state that the 2016 and 2017 events were the largest and longest-lived since 1976. Here, Swart et al. (2018), Cheon and Gordon (2019), Campbell et al. (2019), and Jena et al. (2019) should be cited; note that Swart et al. should be included as they were first to report on the 2017 event.

Phrased as suggested; citations included.

Introduction, Page 1, Line 25: The standard citation for classification of the WSP as an open-ocean or “sensible heat” polynya is Morales Maqueda et al. (2004), not these three papers from 2019.

Correct citation used as indicated.

Introduction, Page 2, Line 35: Replace “contributes” with “may contribute”, as storms will not necessarily lead to ice divergence over Maud Rise (it depends on the particular storm), and both the divergence and mixing were speculative rather than shown directly by Campbell et al. (2019). However, McPhee et al. (1999), Sirevaag et al. (2010), and others have directly measured turbulent mixing over Maud Rise.

Replaced as suggested.

Introduction, Page 2, Lines 49-51: Since the 23-year time series referenced here ends in 2001, this wording is confusing. You can just say that "... the mean sea ice concentration (SIC) for the months of July through November shows... (Lindsay et al. 2004)." Please also omit the "... lacks the open water expanse indicative of a polynya" part. A literal "open water expanse" (i.e., 0% SIC) would only occur in a mean SIC field if a polynya occurred 100% of the time during these 23 years, so your wording does not make sense.

Wording corrected as indicated.

Data/Methods, Page 3, Line 73: Please be more explicit here that the SMOS and SMOS-SMAP retrievals cannot estimate sea ice thicknesses greater than 50 cm.

Section 2.1 paragraph 3 now includes a discussion leading up to why this is the case.

Data/Methods, Page 4, Lines 107-108: This is the incorrect citation. Paţilea et al. (2019) do not show this; this result was found by Heygster et al. (2014).

Correct citation used as indicated.

Data/Methods, Page 4, Section 2.2 (Lines 113-120): Please clarify which satellite mission's data the ASI algorithm is applied to, both here and above on Line 67. Otherwise, the reader will assume ASI is being applied to SMOS or SMAP. I am only familiar with ASI being applied to the SSM/I and AMSR-E/2 sensors, not SMOS or SMAP (Spreen et al. 2008; Beitsch et al. 2014). Provide the appropriate citations for the data.

A clarification, in the first sentence of section 2.2, has been added and states the following: "The ARTIST Sea Ice (ASI) algorithm calculates SIC from the difference between brightness temperatures at 89 GHz at vertical and horizontal polarizations which are retrieved by the Advanced Microwave Scanning Radiometer 2 (AMSR2) onboard theGlobal Change Observation Mission-Water (GCOM-W1) satellite."

Data/Methods, Page 4, Line 123: The findings of your study are not the final word on this interesting and complex question that many have tried to address, so please omit "conclusively" and consider changing "answer" to "investigate".

Changed as suggested.

Data/Methods, Page 5, Lines 135-136: This claim about ERA5 improving on ERA-Interim requires a citation.

Citation added.

Results, Page 5, Line 143: Sea ice thinning does not constitute a polynya. A polynya is, by definition, an area of open water. Please change “the polynya is visible ... but does not open completely” to something along the lines of “sea ice thinning is observed over multiple weeks”. Please also rephrase this in Lines 155 and 163 below.

Phrasing has been corrected as indicated.

Results, Page 5, Lines 143-144: Your Fig. 1 does not compare the SMOS-SMAP retrieval directly to SIC data, so this sentence is not justified in this section. Please delete.

Removed the sentence after the Fig. 1 reference as suggested.

Results, Page 5, Line 147: For reproducibility, it is important to describe and show precisely the “area of interest” that constitutes your averaging region. The coordinates that you cite do not match with the box you drew in Fig. 2 (left) or the subplots in Fig. 2 (right), which are regions irregular in latitude and longitude. Which is your actual averaging region? Please make sure the black box and subplots correspond precisely to the region you use. If it is indeed irregular in lat/lon, you could, for example, give the coordinates of the northwest and southeast corners. Also, please change negative coordinates (e.g., -3.5°E) to the proper values (e.g., 3.5°W).

Northwest and Southeast corner coordinates given.

Results, Page 5, Line 153: Could you be more descriptive in your summary of Fig. 3? For example, you could mention that it shows a broad gradient of SIT encompassing a larger area on all sides of the polynya than shown in the SIC data, which exhibits a sharper gradient of ice concentrations.

The requested description has been added to the main text and the Fig. 3 description has been extended.

Results, Page 8, Line 164: This is not the correct usage of “preconditioning”. With polynyas, preconditioning refers to ocean processes that reduce stratification and/or increase subsurface heat content. This can happen over weeks, months, or years. More accurate here would be to say: “Fig. 5 depicts the Weddell Sea polynya of 2017 as well as the weeks leading up to the event.”

Phrasing has been corrected as suggested.

Discussion, Page 9, Line 180: Since it is not established in the literature that the small 1973 polynya directly produced the 1974-76 event, please say “preceded” instead of “resulted in a much larger iteration of”. Also cite Martinson et al. (1981) and Comiso and Gordon (1987), who mention the 1973 polynya.

Text has been corrected and the citations have been added as follows: “This is similar to the 1970s polynya cases, where the 1973 smaller polynya preceded the larger Weddell Sea polynya visible from 1974 to 1976 (e.g., Martinson et al., 1981; Motoi et al., 1987; Comiso and Gordon, 1998; Cheon and Gordon, 2019).” - the inclusion of words “smaller” and “larger” is meant to inform the reader that the 1973 polynya was much more localized than the 1974-1976 polynya.

Discussion, Page 10, Line 189: Neither low SIC area nor thin SIT area peaked on 4-5 August of 2016, from looking at your Fig. A1. Please fix or omit this.

Erroneous statement omitted.

Discussion, Page 10, Line 191: What do you mean by “shows some variability”? This is quite vague. Be specific, if not quantitative. Additionally, I agree with Reviewer #2 that a correlation coefficient should be presented if you are going to state that two time series are “not very correlated”. Lastly, please fix the grammar in this sentence.

The word “correlation” has been omitted and the vague phrasing has been removed.

Discussion, Page 11, Lines 195-196: Fix the grammar in this sentence, e.g., “... we see that this opening in sea ice, at first minor, eventually paved the way for the Weddell Sea polynya.” Also, do you mean to refer to a small opening that preceded the polynya, or a thinning of sea ice preceding the polynya? I am guessing the latter, but if you intended the former (a small opening preceding the larger opening), please reference Campbell et al. (2019) who also demonstrate this.

Grammar has been fixed. Citation has been added.

Discussion, Page 11, Lines 212-215: Ice may be advected without creating any new open water, i.e., ice advection does not necessarily imply ice divergence. You have not shown or calculated ice divergence, and so what you are claiming (pack ice “broken apart by wind”) is unfounded speculation. However, you could reference other studies that have calculated or discussed ice divergence for the 2016 and 2017 polynyas:Campbell et al. (2019) [see their Methods section “Atmospheric reanalysis” and Extended Data Fig. 5] and Francis et al. (2019) [see their p. 10-11].

The indicated sentence has been omitted in the revision process, for reasons you mention, ice advection was no longer the focus of the discussion.

Discussion, Page 11, Lines 220-221: Reviewers #1 and #2 point out that this statement is not justified by your analysis. I agree.

Addressed in reply to other reviewer's comments. Sentence changed to: "Through the comparison of our SIC data with ERA5 atmospheric data we can speculate what wind conditions are favourable for polynya formation."

Discussion, Page 11, Lines 221-222: For your statement regarding 13 September 2017, please cite the prior studies, e.g., "..., corroborating the findings of Campbell et al. (2019) and Francis et al. (2019)."

Cited in a manner that was suggested.

Discussion, Page 11, Lines 222-224: Do you have a figure showing this? If not, please note in parentheses: "(not shown)".

Notice added.

Discussion, Page 11, Lines 228-230: This wording ("instead of") implies that you use SMOS before 2015 and SMOS-SMAP from 2015 onwards in Fig. 1, which contradicts your Fig. 1 caption. Please clarify your wording to make clear which is the case.

Grammar fixed; clarification made. Sentence changed to "Lastly, we use the SMOS SIT retrieval instead of the combined SMOS-SMAP to also include years before 2015 (the year when SMAP was put into orbit) to make a consistent 11 year SMOS SIT time series over the months of July, August, September and October (Fig. 1) that fully includes the freezing periods of the relevant region over the years."

Fig. 8 caption, Page 13: See my comment above on the Abstract regarding the earlier sea ice opening on September 3, 2017. I would say that the polynya "rapidly expanded" on September 13, rather than opened for the first time.

Phrasing changed as suggested.

Conclusions, Pages 14-15: In this section, I suggest that you consider discussing how your findings relate to those of Heuzé et al. (2021), whose recent analysis of past polynya events at Maud Rise is highly relevant. Could the SIT data you present offer an "early detection" system for polynyas, as their study aims to develop? In other words, do you find that SIT anomalies consistently precede sea ice openings at Maud Rise? How much lead time prior to a polynya opening could a SIT-based detection system offer?

That was not the intent of our study thus we can speculate on the usefulness of such an early detection system. Despite this, we now comment on this concept in lines 241-249 as outlook.

Conclusions, Page 14, Line 241: Reference my earlier comments regarding the opening date of the 2017 polynya.

Phrasing changed as indicated earlier.

Conclusions, Page 14, Line 255-256: Campbell et al. (2019) also show this using in situ ocean observations, as well as quantifying the rate of heat loss during the 2016 polynya (see their Fig. 3 and Extended Data Fig. 7).

Citation has been added.

Conclusions, Page 14, Line 261: Since you do not quantitatively test correlations, please change “has the most direct correlation” to “is most directly connected” or something similar.

Phrasing changed as suggested.

Conclusions, Page 14, Lines 264-265: This is not a good summary of the processes that Francis et al. (2020) arguecontributed to formation of the 2017 polynya. Please fix. Their study focuses on how atmospheric rivers changed the energy balance of sea ice and the overlying snow cover to favor surface ice melt and thinning.

Summary changed and corrected to the following: “Also worth mentioning is the work done by Francis et al. (2020) that demonstrate the impact of moisture-carrying atmospheric rivers during polynya years which in addition to increasing snow fall, which effectively decouples the sea ice from the cold atmosphere once precipitated, brings clouds that trap the outgoing long-wave radiation locally resulting in further ice melting.”

Conclusions, Page 14, Lines 271-272: The fact that the SMOS-SMAP retrieval is influenced by SIC means that, by definition, it is not independent of SIC. Please change to something along the lines of “an additional source of Information”.

Phrasing changed as suggested.

Conclusions, Page 15, Lines 282-283: I don’t see how daily SMOS-SMAP snapshots allow monitoring “on a more frequent basis” than existing daily SIC data. Delete.

Erroneous text deleted.

Conclusions, Page 15, Line 290: I agree with Reviewer #1—the phrase “purely-open ocean polynya” is confusing and, in any case, neglects past work that has discussed atmospheric influences on the polynya.

Addressed in reply to other reviewer's comments. Sentence has been changed to: "In conclusion, through comparisons between SIT and ERA5 data we corroborate the idea that the Weddell Sea polynya is not purely ocean-driven and instead also facilitated by direct atmospheric forcing."

Data Availability / Acknowledgements, Pages 15 and 17: Please move the appropriate URLs, DOIs, and information about other data sources (e.g., ERA5) to your Data Availability statement; they do not belong in the Acknowledgments.

Data URLs moved oved as requested, however, an acknowledgement for ERA5 will remain.

Appendix A2 / Fig. A2, Pages 16-17: I'm not sure what you are aiming to illustrate with Fig. A2. It is not referenced in the text of your paper. If you want to include it, please discuss and reference it somewhere in the text; if not, Appendix A2 can be removed. Note that "snippets" is not a formal term; change to "images" or similar.

This figure is now mentioned in the main body of the text as follows: "For a better resolved image of both the Weddell Sea polynya of 2017 and the SIT anomaly of 2018 see Fig. A2."

Technical corrections / typographical comments:

All technical corrections/suggestions were accepted and were implemented as suggested.

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