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 all the hard work you put in your in-depth review that will undoubtedly lead to a better manuscript.

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 will go with the latter option and standardize 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 the time reviewing the suggested literature. As a result, previously erroneous speculations presented in the manuscript have been rewritten and improved. For the introduction, we will take your suggestion and reorganize the section as you suggest. We thank you for pointing out all the manuscripts that have not been properly cited in our work. All of the citations you pointed out were added where necessary.

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 papers you mention will be referenced where appropriate.Thank you for pointing this out.

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.

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.

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.

Thanks to your, and reviewer 1’s, comments, most if not all 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. Thus, we thank you for helping us correct this issue as well as cite past studies on the subject.

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.

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.

We apologize for the sentences that were copied without sufficient rephrasing of the text. We have now put them in our own words and therefore thank you for pointing this issue out to us.

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.

This point has been noted and we will update our introduction section accordingly.

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 information will be included in the updated and revised Data/Methods section.

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.

    Your concerns are noted and the limitations you speak of will be made clearer.

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 are now shifting the focus from taking the SIT retrieval at face value, and analyzing the retrieved signal as apparent SIT. 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.

   While we will explicitly refer to the Figure 9a from Paţilea et al. 2019 for reference, we decided low-SIC based SIT uncertainty derivation to be unnecessary in view of our decision. 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 will be rewritten and revised in accordance with the shift from SIT to apparent SIT.

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.

   The clarification you're asking for will be added.

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.

   We did not mean to use them to validate the product. We will rephrase accordingly.

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 section has already been rewritten after reading through all the suggested literature as well as re-reading through manuscripts that were cited initially. We thank you for your insights that have put us on the right track and expect the speculative discussions to improve in quality as a result.

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.

After reviewing the relevant literature, we now know what you are referring to. The discussion section, as mentioned before, will be rewritten in view of the currently present inaccuracies.

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 will be deleted or reworded as necessary.

Lastly, we thank you for prompting us to delve deeper into the meaning of our results and significantly improve the outlook of the manuscript to also include what we propose the dataset to be used for as well as include further comments on the 11-year SMOS record.

Specific Comments Reply:

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."

    Done.
- 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).

Corrected.

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.

This has also been addressed in reply to other reviewer’s comments. Other occurrences of polynyas in that area will be mentioned with corresponding references.

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.

Citation corrected.

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.

The revised version of this section will include explicit mentions of this limit as well as why it's there; in addition to changes mentioned in the General Reply.

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).

Citation corrected.

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.

Refined description of ASI data. It was also requested by the other reviewers.

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 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 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.

Citations added.

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 fixed. Citation 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].

Other studies referenced.

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?

This will certainly be added to the manuscript, thank you for the suggestion.

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 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 will be mentioned in the main body of the manuscript.

Technical corrections / typographical comments:

All technical corrections/suggestions are accepted and will be implemented as suggested.

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Lindsay, R. W., D. M. Holland, and R. A. Woodgate: Halo of low ice concentration observed over the Maud Rise seamount. Geophys. Res. Lett., 31, L13302, doi:10.1029/2004GL019831, 2004.

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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.