Summary:

This manuscript presents an analysis of winter sea ice thickness in the Weddell Sea using retrievals from the space-borne SMOS and SMAP passive microwave sensors. The study period spans 2010-2020 and focuses on sea ice variability in the vicinity of the Maud Rise seamount, located in the eastern Weddell Sea. A key finding is that this region features substantial sub-seasonal variability in sea ice thickness that is poorly captured by traditional sea ice area/concentration products. The authors also showed that in 2018, there was a period of large-scale sea ice thinning near Maud Rise that was reminiscent of the lead-up to the major polynya event in 2017. For both 2017 and 2018, they show that the initiation of strong basal melting coincided with strong surface wind events.

General thoughts:

This paper presents groundbreaking results that reveal the nuances of sea ice thickness variability in the Weddell Sea. The Southern Ocean community has been eagerly awaiting reliable estimates of Antarctic sea ice thickness and the dataset featured here is a very promising step towards that goal. This work also provides closure to the 2016 and 2017 Maud Rise polynya events, which seemingly came to an abrupt end in 2018. Many oceanographers (myself included) expected the polynya to return in 2018 but were taken by surprise when the polynya did not reappear despite favorable upper ocean conditions. The authors showed that these events did not come to an abrupt end and that the sea ice field was in fact on the cusp of producing another polynya in 2018. Overall, it was quite satisfying to see these results as they confirm my long-held suspicion that the sea ice field over Maud Rise undergoes cycles of substantial thinning and re-growth in the winter that are not captured by estimates of sea ice area.

Though the scientific significance of this manuscript is high, the paper has several shortcomings that I believe need to be addressed before publication can be recommended. Below, I try to summarize my two major concerns:

i) <u>Acknowledgment of relevant past studies is inadequate</u>: 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.

ii) <u>Description of SMOS-SMAP sea ice thickness retrieval is a bit lacking</u>: 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.

Besides these two major concerns, I have less serious concerns regarding the presentation of data and the writing itself. After these revisions are implemented, I believe this paper will be a widely-read and significant contribution to the Southern Ocean literature.

More detailed comments:

- 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 the late 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.

- 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).

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

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

- 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.

- 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?

- 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.

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

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

- 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.

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

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

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

- 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.

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

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

- 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.

- 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.

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

- 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.

- 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.

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

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

- 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.

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

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

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

- 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.

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

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

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.

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.

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.

Typos and other miscellaneous formatting issues:

- Line 65: "for" should be capitalized.

- Line 67: "sea ice thickness of ice" is a redundant phrase.

- Line 70: SMOS and SMAP are already defined

- Line 71: "which allows to provide" I think this is a typo.

- Line 81: "...SMOS-SMAP thin ice sea ice thickness..." Redundant. Delete first "ice".

- Line 87: typo. "Because of to the 12 hour..."

- Line 143: "the" is repeated.

- Line 203: "the" is repeated.

- In the references, many of the DOI links begin with "https://doi.org//https://doi.org".

References:

Francis et al. (2019): Polar Cyclones at the Origin of the Reoccurrence of the Maud Rise Polynya in Austral Winter 2017. DOI: https://doi.org/10.1029/2019JD030618

Goosse and Fichefet (2000): Open-ocean convection and polynya formation in a large-scale ice–ocean model. DOI: https://doi.org/10.1034/j.1600-0870.2001.01061.x

Kurtakoti et al. (2020): On the Generation of Weddell Sea Polynyas in a High-Resolution Earth System Model. DOI: https://doi.org/10.1175/JCLI-D-20-0229.1

McPhee et al. (1996): The Antarctic Zone Flux Experiment. DOI: https://doi.org/

10.1175/1520-0477(1996)077<1221:TAZFE>2.0.CO;2

McPhee et al. (1999): Ocean Heat Flux in the Central Weddell Sea during Winter. DOI: https://doi.org/10.1175/1520-0485(1999)029<1166:OHFITC>2.0.CO;2

Wilson et al. (2019): Winter Upper-Ocean Stability and Ice–Ocean Feedbacks in the Sea Ice–Covered Southern Ocean. DOI: https://doi.org/10.1175/JPO-D-18-0184.1