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Interactive comment on "Spatio-temporal variability and decadal trends of snowmelt processes on Antarctic sea ice observed by satellite scatterometers" *by* Stefanie Arndt and Christian Haas

Anonymous Referee #1

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The manuscript builds on the study of Haas et al. (2001) on the analysis of spaceborne scatterometer data for investigating snow melt. The work compiles a time-series of melt onset information from ERS-1/2, QuikSCAT, and ASCAT for the 1992 to 2015 period. The retrieved dates are then compared to those derived from passive microwave data. The analysis presents new and relevant information about snow on Antarctic sea ice and the capability to remotely sense snow conditions on Antarctic sea ice. There are several aspects of the analysis that require clarification, more details and revised interpretation based on previous studies. Please find detailed comments below.

Page 1, line 6. From a broader view, the presence of snow would be the key driver, wouldn't it?

Page 1, line 13. Metamorphism is an umbrella term for several types of metamorphic processes of snow. What exactly do the authors mean by metamorphism? The use of it in the manuscript is ambiguous at times. I recommend adding "melt" or "freeze-melt" before "metamorphism" throughout the manuscript to differentiate it from other metamorphic processes.

Page 1, lines 23-26. I suggest rewriting this section since it's an over-extension of the results. The conceptual model hypothesizes that the evolution of seasonal snow temperature profiles could affect different microwave bands. It's also important to define the environmental conditions that the conceptual model is limited to. From what's been described, it seems like the conceptual model would be applicable to a snowpack with low density, no damp/saline basal layer, no internal icy layers or lenses, somewhat uniform vertical grain structure, and over perennial ice where high-frequency diurnal temperature fluctuations occur simultaneously with a slow, steady increase in mean temperature.

Page 2, line 10 and throughout the manuscript. Please specify which Haas et al. 2001 paper that you refer to.

Page 2, line 10. How do variations in snow properties affect the mass budget of the ocean?

Page 2+ Snowmelt onset in the Antarctic is described as more subtle than the Arctic. What is meant by subtle? Previous studies describe warm, marine cyclones that bring dramatic temperature swings and/or rainfall throughout the year, and with increasing frequency going into summer. The induced melt from these events is not subtle and typically results in a more structurally-complex snowpack. Similarly, the manuscript overly generalizes Arctic snowmelt, as on page 21 line 9-10, "...warms very rapidly throughout the entire snow column during melt onset..." What observations support these statements? Snowmelt is often not as rapid and continuous in the Arctic as described in the manuscript. Peng et al. 2018 is an insightful example with their definitions of melt onset periods. Studies have shown numerous snowfall and freeze events during spring and summer, which highlights the discontinuous nature of melt (and freeze) that seems to occur in all snow environments.

Page 2, line 31. In contrast to what?

Page 2+. Salinity affects radar backscatter. Previous observations not only show brine wicking up to 15-20 cm into the Antarctic snowpack from its base, but that as a whole the Antarctic snow cover is saline. I encourage the authors to consider the effects of salinity on the retrieved dates and adding in a discussion on this topic in the manuscript.

Page 3, line 2. Arndt et al. 2016 seems like the wrong reference here.

Page 3, lines 17-18. If flooding is indeed an important mechanism for Antarctic snow and sea ice as described in Massom et al. (2001), I recommend adding more discussion on what the potential effects of flooding are on the results. Here and elsewhere in the manuscript, flooding is swept “under the rug” so to speak by suggesting that it only occurs right before the sea ice cover disintegrates or is limited to the edge of the sea ice pack.

Page 3, line 23. Superimposed ice. Do we know that this it is a wide-spread phenomenon during snow melt onset? My understanding is that a substantial amount of snowmelt is required before the meltwater can fully percolate down to the ice surface. I suspect superimposed ice would occur after snow melt onset for this reason. The observations in Haas et al. 2001 were ∼2 months after the snow melt onset dates shown here, so it’s not clear if the presence of superimposed ice can be used to interpret the backscatter for identifying melt onset. There may be comparable situations in the Antarctic where superimposed ice does not form at all, see Polashenski et al. 2017 for an Arctic example. Based on the literature, rainfall may also be important to consider in Antarctic snow.

Page 4, line 3. “Adjusted” would be a more appropriate word that “corrected” here and elsewhere in the manuscript.

Page 7. The sample size is limited to a pixel for each location to reduce the variability associated with different ice conditions. How sensitive are the results to one pixel vs. a multi-pixel average? I would suspect that variability is larger for a single pixel due to the advection of ice with differing properties. An eight-neighbor mean may be more stable.

Page 7, line 10. It would be helpful to clarify that the sea ice concentrations are from the Bootstrap algorithm.

Page 5, lines 15-16. What information was used to determine which areas were predominantly seasonal and perennial ice? Is there the possibility that some years had a mixture of ice types at the designated sites?

Page 5, lines 18. Anderson, Bliss, Peng appear to be under-referenced with regard to melt onset detection from passive microwave data.

Page 7, lines 26-29. How would a 70% sea ice concentration threshold remove flooded ice from your sampling? How are ice concentration and flooded sea ice related?

Page 8, figure 4. It would be helpful to show the sea ice concentration here and either as additional figures or in supplementary information for figures 2 and 3 given its influence on backscatter. How were the start and end points of the bolded solid lines determined?

Page 9, lines 1-11. How much of this is speculation? Were coincident in situ observations linked with observed changes in backscatter? Please clarify in the manuscript.

Page 9, line 10-11. Please specify that you mean a positive albedo feedback. The manuscript neglects here and elsewhere the possibility of stopping surface melt due to fresh snowfall.

Page 9, line 20-22. How was October 1st determined? How were the 2 dB and 3 dB thresholds determined? Are the results sensitive to these choices?

Page 9, lines 26-30. What fraction of the time-series had indeterminable melt dates for perennial and seasonal ice? It would be helpful to put those numbers in the results section.

Page 10, line 5. It would be helpful to give more detail on what the "regionally adaptive" approach does.

Page 10, lines 18-20. It would be helpful to give more detail here. What is the iterative algorithm converging on exactly? Is a priori information on thresholds needed?

Page 12, line 14. Is 7.66 dB different from the value found in Haas et al. 2001? If so, why?

Page 13, Section 3.2. Similar to an earlier comment, approximately what fraction of the time-series had detectable melt onset? This can help provide the reader with context on the limitations (and possibilities) of this approach over seasonal ice.

Page 17, figure 9. It would be helpful to give the sample size of each mean difference, either in the figure or in a table, so that readers can appropriately interpret the spread.

Page 18, lines 1-3. I suggest rewording this sentence. As it's stated, it sounds like perennial ice has larger brine volume at the surface, which is probably not what you mean.

Page 18, lines 7-9. Do you have a reference for this statement?

Page 18, line 25. "Instead, we suggest…" I recommend changing this to: "Instead, other studies have shown…" since this analysis does not show results on these topics.

Page 19, line 3. "…seasonal mass balance of Antarctic sea ice in the future." Based on the results, isn't this approach only appropriate for perennial sea ice? If so, it would be good to make that clarification in this ending paragraph.

Page 19, Section 4.3. This is an interesting idea, but it misses some fundamental characteristics of snow, the most significant being light penetration in snow vs. blue ice, the existence of a saline, damp layer at the base of the snowpack and icy layers and lenses within the snowpack. The Brandt and Warren (1993) study shows that visible wavelengths are not absorbed at depth in a snowpack, but are scattered back to the surface. Near-infrared wavelengths only get absorbed in the top few millimeters of the snowpack. The study then describes optimal conditions where sub-surface melt could be important. These are low-albedo ice like blue ice and low-density snow, like depth hoar. Both conditions are not typical of snow on Antarctic sea ice. The description on page 20, lines 11-16 must be a misinterpretation of the Brant and Warren analysis and needs revising. Related, several studies have since shown technical issues with radiative heating of sensors, such as in Cheng et al. 2003, making observed sub-surface temperature increases somewhat dubious. Secondly, there is a wealth of papers that show the widespread occurrence of icy layers and a damp, saline basal layer in snow on Antarctic sea ice, in contrast to an assumption of dry snow as on page 20, line 33. This damp layer, as well as internal icy layers within the snowpack, greatly modify the electromagnetic signature of snow, its temperature gradient and snow metamorphism. I encourage the authors to give these aspects consideration and incorporate them into the proposed hypothesis. If the hypothesis in Section 4.3 conflicts with typical characteristics of Antarctic snow, then explicitly state that it does and describe specifically which environmental conditions the hypothesis is limited to. Although simple, the schematic in Garrity (1992) is informative and may help with this. For figure 10, it would be helpful to overlay snow grain symbols so that readers can have a better idea of which melt-induced characteristics you're referring to in the snowpack for each stage. The WMO and Colbeck (1991) would be useful references for this.

Page 21, lines 24-27. Could you provide some references to support these statements? Also, what is meant by "the most potential surface changes?"

Page 21-22, lines 30-32/lines 1-2. How do we know this is true? Are there references to support these statements?

Page 22, line 3. It's stated that the results obtained in this study demonstrate the potential to observe snow processes at different depths from space. This is not true. However, the study does hypothesize that this could be possible, which is different from a demonstration. Please correct this for clarity.

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