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.

We highly appreciate the great work that the reviewer put into revising our manuscript. This work is really excellent and we realize that he / she is really familiar with this field of research and has a great expertise to make most useful comments and suggestions. We thank the reviewer for the very critical questioning regarding the described relevant and dominant processes causing seasonal changes in the properties of the Antarctic snowpack. To overcome these missunderstandings, we added more explanations in the respective sections as pointed out in the following explicite responses.

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.
We have added "destructive" to describe what kind of metamorphism we refer to. Other than that we believe that it is clear from the context that this is what is meant, and don't feel that it needs to be repeated frequently.

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.
We have added "on thick Antarctic snow" to the abstract to indicate that our model may not be applicable to all sea ice regions worldwide. Other than that we don't feel that much more information should be given in the abstract., although we have added more specific information later in the text.

Page 2, line 10 and throughout the manuscript. Please specify which Haas et al. 2001 paper that you refer to.
Throughout the manuscript as well as in the reference list we carefully distinguish between Haas et al, 2001 and Haas, 2001.

Page 2, line 10. How do variations in snow properties affect the mass budget of the ocean?
We agree that the given sentence is misleading and therefore adapted it towards the "mass budget of sea ice".

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 de- scribed 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.
Which studies does the reviewer refer to? Our statements are meant in general and address the fact that no melt ponds are observed in the Antarctic, in contrast to the Arctic. It is clear that there are instances when melt in the Arctic is retarded by new snow events or cold spells. However, this does not change the general recognition that snow melt in the Antarctic is much more subtle/slow/sporadic than in the Arctic. A more detailed deicussion of differences between Arctic and Antarctic snow processes is beyond the scope of that paper. We have clarified the text:

However, on Antarctic sea ice, the retrieval of snowmelt onset is more challenging because thawing and melting are weaker and more sporadic than in the Arctic. There is widespread occurrence of diurnal thaw-freeze cycles (Haas et al., 2001;Nicolaus et al., 2006;Nicolaus et al., 2009), or the snow may only thaw during the passage of warm marine cyclones, with the snow refreezing shortly after (Willmes et al.,xxx). These thaw-refreeze events cause strong,

destructive snow metamorphism. Under more intensive melting conditions, snow changes from the pendular to the funicular regime (e.g. Denoth, 1980) where the liquid snow melt water percolates through the snowpack to lower, colder layers or to the ice surface where it refreezes to form superimposed ice (Tison et al., 2008;Haas et al., 2008;Haas et al., 2001;Nicolaus et al., 2009;Willmes et al., 2009).

Page 2, line 31. In contrast to what?
'In contrast" was meant to distinguish between dominant processes in winter (snow-ice formation) and spring/summer (superimposed ice). However, to reduce the confusion, we removed the discussion of flooding and snow ice from this section and have included it in our later discussion of winter processes preceding the transformation of first-year ice to perennial ice during melt onset.

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.
We are well aware of the saline nature of some snow on first year ice, and that there can be widespread flooding. These properties are also responsible for the ice's low backscatter in winter. However, once the snow warms the brine typically drains and snow salinity measurements in summer show negligible salinity throughout. The text has been rewritten significantly to clarify these points.

Page 3, line 2. Arndt et al. 2016 seems like the wrong reference here.
We agree and deleted it.

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.
See above, we have consolidated the text to better separate between winter and summer properties and processes, and have described that salty snow and flooding are responsible for low backscatter during winter. We have then clarified that the snow desalinates during spring and that negative freeboard is uncommon on perennial ice.

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.
We agree with the reviewer that superimposed ice is not responsible for the initial backscatter rises. However, together with icy snow and ice layers it contributes to maintaining high radar backscatter by the end of the summer. We have clarified this in the text by restructuring and adding a few words. We added a few words about passage of warm cyclones to include cases of rain fall. Again, these events are mostly sporadic and typically lead not to strong, accelerated melt nor the formation of melt ponds.

Page 4, line 3. "Adjusted" would be a more appropriate word that "corrected" here and elsewhere in the manuscript.
We agree and adapted it.

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.
We had actually conducted our analysis for individual pixels and groups of 3x3 pixels. We agree that a larger region of 3x3 pixels will provide more representative results for the respective areas. Therefore, we are now reporting the results of the analysis of the 3x3 pixel regions. However, this has no effect on the presented results in the manuscript but the given dates shifted slightly by 1-2 days back or forward.

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

Page 5, lines 15-16. What information was used to determine which areas were pre- dominantly seasonal and perennial ice? Is there the possibility that some years had a mixture of ice types at the designated sites?
The given locations were chosen to agree with those of Haas (2001) in order to ensure both a proper comparison between both studies and a reasonable continuation of the given time series. However, it can not be ruled out that single

points are not in all given years covered by a MYI regime. In such years there may not be any results because our algorithm does not work for thin, deteriorating FYI. We have stated the percentage of successful retrievals on page xxx: Overall, pre-melt and melt onset dates were retrievable for 79% and 64% of all analyzed pixels. For the seasonal sea-ice regime, pre-melt and melt onsets were obtained for 46% and 26% of the analyzed pixels.

Page 5, lines 18. Anderson, Bliss, Peng appear to be under-referenced with regard to melt onset detection from passive microwave data.
Thank you for these suggestions but we prefer to limit our citations to a few, most relevant and recent publications.

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?
Clarified: This avoided contamination of results by wind-roughened water (Drinkwater and Liu, 2000), and effectively eliminated regions of deteriorating, thin ice where surface flooding and break up into small floes and brash ice may occur, e.g. in the marginal ice zone, with competing effects on backscatter evolution.

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?
For Figure 4 the grey shaded area indicates the part of the time series with sea-ice concentration less than 70%. As given in the figure caption, the bold lines indicate the time period included in the transition retrieval (01 Oct to 31 January). However, we added these dates in order to make this clearer.

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.
Most of our observations were carried out during the ISPOL drift station in 2004/05. We added more references to that part.

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.
We agree and clarified that.

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?
The thresholds are based on the average rise of the backscatter values during the spring/summer transition. This is stated in the last sentence of the paragraph. It is unlikely that melt onset would occur before.

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.
We agree and added the percentage of the respective successful retrievals: Overall, pre-melt and melt onset dates were retrievable for 79% and 64% of all analyzed pixels. For the seasonal sea-ice regime, pre-melt and melt onsets were obtained for 46% and 26% of the analyzed pixels.

Page 10, line 5. It would be helpful to give more detail on what the "regionally adaptive" approach does.
The working principle of the regionally adoptive approach is described right after the approach is mentioned.

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?
The iterative threshold selection algorithm derives from the given $d\sigma^0$ values per pixel the optimized threshold in order to distinguish between summer and winter conditions. We slightly adjusted that sentence in order to make this clearer.

Page 12, line 14. Is 7.66 dB different from the value found in Haas et al. 2001? If so, why?
Numbers are slightly different from the ones given in Haas (2001) as the amount of analyzed pixels slightly differ between both studies.

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.
We agree and provided the respective fraction of detectable dates.

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.

We agree and added these numbers to the figure.

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.

Reworded: Instead, the younger and thinner seasonal ice is warmer and more salty than perennial ice, and has larger brine volume at the snow/ice interface causing low backscatter through winter

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

Yes, we added a respective reference: (Martinson and Iannuzzi, 1998).

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.

We agree and added that.

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.

We agree and specified that we are referring to perennial sea ice only.

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.

We disagree with the reviewer and are confident that we do not misinterpret the Brant and Warren study. We are clear about the importance of extinction coefficients, and the debate about how strong the sub-surface temperature maximum can be. However, we have added a few words to actually suggest that properties of metamorphic snow on Antarctic sea ice can be comparable to the properties of blue ice (which is essentially clear ice with few air bubbles), in as they have large grains lowering the snow's specific surface area (SSA), and ice layers which are essentially clear ice with few air bubbles.

Changed text: Similar internal melting is observed in, e.g., blue ice regions on the Antarctic ice sheet (e.g. Brandt and Warren, 1993;Cheng et al., 2003;Liston and Winther, 2005). While the magnitude of the difference between the snow surface temperature and the sub-surface temperature is debated (Brandt and Warren, 1993), the subsurface temperature maximum depends on the snow's extinction coefficient and is larger with denser snow, with larger grains lowering the specific surface area (SSA), and in the presence of ice layers, which are typical for perennial Antarctic sea ice (Nicolaus et al., 2009).

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.

We acknowledge the fact that temperature measurements in warm snow in a radiative environment can be challenging. However, we do not rely on measurements for developing our conceptional, qualitative model, and Cheng's modeling and other considerations suggest that the occurrence of subsurface temperature maxima in the snow are quite possible. This is also demonstrated by the subsurface melting on blue ice, even though albedo and extinction coefficients may not be directly comparable with sea ice as discussed above. As witnessed by the authors on Novo airbase, water pockets under a surface layer of ice are quite common during summer, resulting exactly from the vertical profiles of radiation absorption and emission discussed here.

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.

We feel that throughout the text and in our replies above we have argued sufficiently and have changed the text sufficiently to acknowledge the presence of damp, saline snow in winter, but have also convincingly explained that saline snow does not play a role in summer. On the contrary, ice layers, which are mentioned by the reviewer along

with saline snow although they usually do not form concurrently in the same seasons, do contribute to the observed backscatter increases and most likely also play an important role in the development of a subsurface temperature maximum.

Although simple, the schematic in Garrity (1992) is informative and may help with this.
We are well aware of the work of Garrity and have worked with her in the field. However, we find that work little useful for our study as it describes only the short time of snow wetting during short melt events, and does not consider the temporal changes throughout the melting period and refreezing. The reported floes were in the marginal ice zone. We suspect that most of that ice did not survive the summer.

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.
Unfortunately, there is not one symbol for metamorphic snow but they are distinguished for different grain types which would be misleading at this stage. We therefore decided to keep the given symbols but added horizontal arrows between the day- and nighttime temperature profile in order to make the thaw-freeze cycle clearer.

Page 21, lines 24-27. Could you provide some references to support these statements? Also, what is meant by "the most potential surface changes?"
We have rephrased the sentence and also added a reference for the role of the ocean in contributing to Antarctic sea ice extent variability: However, we believe that they will not, based on the facts that previous changes of ice extent during our study period did not strongly affect melt onset dates as mentioned above, that those recent changes probably have a strong contribution from oceanic processes (Turner et al., 2015), and that most of the potential surface changes related to ice extent fluctuations may occur in the seasonal ice zone or closer to the marginal ice zone where our algorithm is not applicable.

Page 21-22, lines 30-32/lines 1-2. How do we know this is true? Are there references to support these statements?
We don't understand this comment. The paragraph is a summary of our findings and conceptual model and has been supported by the discussion throughout the text.

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.
Rephrased: Based on the results obtained here, we suggest that there is a potential to observe snow processes at different depths from space, opening new avenues for studies of energy and mass budgets of snow on sea ice in the Southern Ocean.