Response to RC1:

Note: We cite the reviewer's comments in blue and have numbered them

We again thank the reviewer for the critical eye and for the very detailed and constructive criticism.

Some aspects of the manuscript have improved compared to the version I reviewed. I still have a number of concerns, though, that make me to suggest another round of major reviews. The reasons for that I lay out in my general comments, further detailed by my specific comments.

General Comments:

GC1: The authors stated in their response to my 1st review that this is a proof-of-concept study and used this as an argument to keep a lot of the content of the manuscript at a level of depth where is was before. I therefore ask the authors to reflect the nature of this study in their title, in the abstract and also in the conclusions.
See item (1.)

GC2: There are certain elements of the methodology that are still not described sufficiently well. To these belong how the initial MYI distribution is found, or how ice drift information is employed. In addition, the intercomparison of TBs and NRCS values from the sampling areas and periods with independent values of these parameters needs to be improved. At least the obvious discrepancies between the values used and those published in the literature should be mentioned properly and taken into account when discussing the limitations of the approach and the potential way ahead.
About the initial MYI distribution, see item (16.a) below. How ice drift information is employed is quite explicitly explained in section 2.2. On the discussion of TB and NRCS values from our distributions vs. literature see (GC3) below, as well as items (19.), (20.), (20.a) and (21.).

GC3: The interpretation and discussion of the results still requires a more thorough incorporation of the physical properties of sea ice and its snow cover and their influence on the microwave properties. The discussion appears still to be biased to much towards improving the correction scheme rather than improving ECICE and the selection of adequate sampling areas and periods for the adoption of ECICE from Arctic to Antarctic conditions.
ECICE is a mathematical scheme, which does not care about physics, and in this sense it cannot be improved using knowledge of physical properties. What can and has to be improved is the selection of sampling areas – we have addressed that point already in the first revision, but we will elaborate more on that (cf. items (16.a), (16.b), (18.b)). But certain effects. namely temporal changes of the scattering and radiometric properties of the ice types, cannot be taken into account by better sampling, and this is exactly what the correction schemes are meant for. So one cannot say that „ECICE should rather be improved instead of the correction scheme“ – both are important. We admit that in the original manuscript, we were biased towards the correction schemes, but we are now giving more room to the sampling problem as already mentioned.

GC4: There is a number of other smaller issues where the information provided is either not correct or needs to be complemented or where a more critical discussion of the own results appears to be useful for a more complete understanding of the main message of the manuscript.
See „Specific Comments“

Specific Comments:

(0.) Title and abstract: You have decided to not build your paper on a solid review of our understanding of the different physical properties of the Antarctic sea ice compared to the Arctic sea ice and their implications on active and passive microwave satellite data. Fine. You have also decided to keep out expertise about snow on Antarctic sea ice. In that case, however, I cannot accept this paper without the following suggestions for amendments in the title and the abstract.

(1.) The title is far too general and points into the wrong direction. As you stated in your comments to my 1st review this is a "proof-of-concept" study. This should be mentioned in the title.
We have modified the title, it now reads: „First results of Antarctic sea ice type retrieval from active and

passive microwave remote sensing data“. We do not use „proof-of-concept“ in the title as we think the paper is more than that: It is a proof of concept and the presentation and discussion of first results.

(2.) You use ECICE and you discriminate young ice from first-year ice from multiyear ice - similar to what ECICE does in the Arctic - without modifying ECICE for the Antarctic for the reasons laid out in your response to my 1st review. Please mention ECICE in the title (there are Arctic ECICE papers, so it does not harm to mention it here). Hence a title such as: About a proof-of-concept study applying the ECICE algorithm to discriminate Antarctic multiyear ice from younger ice types" would be much more to the point.

We do not think is is a good idea to use an acronym like ECICE (which is not generally known, i.e., cannot be found in dictionaries) in the title of a paper.

(3.) In the abstract I suggest you write that ECICE uses probability distributions that decouple its application from the need of a deeper understanding and consideration of the emissive and backscattering microwave property differences between the two polar regions. This would lend much more credibility to the abstract and would fit much better to the content of the paper. Note that in L8-10 you explicitly write "Due to differences in physical and crystalline structural properties of sea ice and snow between the two polar regions, it has become difficult to identify ice types in the Antarctic". Since you do not deal with an exhaustive investigation of the different properties on the AMW and PMW signals but still come up with a solution - which is only possible thanks to the nature of the ECICE algorithm using probability distributions - I feel it is mandatory to tell the reader this piece of information, i.e. that you propose to, as a first proof-of-concept, apply a method based on probability distributions of the involved AMW and PMW data. See also my next comment.

Do we really have to justify that our algorithm (ECICE) “only” takes distributions of the input parameters for the surface type and does not need explicit physical knowledge of sea ice? Most methods for the retrieval of ice concentration do not even use distributions, but merely use *one value* per input channel per surface type, and no further physical details. Using distributions instead of single values does seem a bit closer to the physical reality. – We have inserted a sentence about the nature of ECICE, now L11/12.

(4.) The sentence "Until recently, no method ... time scales" in L10/11 should be deleted to my opinion. You did not develop a new method here. You applied a method that is known to work for Arctic conditions to Antarctic conditions to see whether it works.

Well, we meant that no other method has provided data for monitoring the distribution and temporal development of Antarctic ice types, particularly MYI throughout the freezing season and on time scales of several years. We admit that you can argue whether ECICE plus corrections schemes can be viewed as two different methods for the Arctic and Antarctic – if we replace „method“ by „retrieval scheme“ we can certainly say so. Note also that ECICE/correction schemes for retrieving Arctic MYI was published quite recently (*Ye et al.*, 2016a,b) – depending on how you define „recent“. We now say: „ […] no retrieval scheme was ready for monitoring the distribution […]“, now L8/9

(5.) I also strongly recommend to re-formulate the sentence "Although there are ... sea ice types" in L16/17. This is not the first time we learn about the evolution and dynamics of Antarctic sea ice types. I would rather say: "Our results of applying ECICE to Antarctic sea ice conditions for the first time demonstrate its potential for Antarctic sea ice type discrimination. They also confirm existing knowledge about typical spatial pattern in the sea ice type distribution and their movement."

Thank you for your consideration.

This is not about the typical patterns and yearly cycles, but about the possible interannual variations. The typical patterns are known, but the year-to-year deviations from the typical pattern are hardly known. We have made it clear by adding the word “interannual” here (now in L16). See also next item.

(6.) L39/40: "Unlike the Arctic ... still unclear" --> I object to this statement. It is well known where sea ice typically survives summer melt. It is well known where it usually drifts. It is well known that a lot of the sea ice is either formed along the marginal ice zone in the so-called pancake ice cycle or in the ubiquitous coastal polynyas from where it drifts north or northeastward. Also the location and age of landfast sea ice is well known.

We agree and have rewritten the sentence. It now reads „While the general distribution and yearly cycle of the different sea ice types in the Antarctic are known to some extent, details, the interannual variations and possible long-term trends are still unclear.“ – now L39-41

(7.) L41-42: "In the austral summer ... 2012a)." --> What do studies by Hobbs et al. (2015) and Parkinson and diGirolamo (2021 and before) state in this regard?

They also mention an overall slight increase (until 2014), but with regional differences. Both Hobbs et al. (2015) and Parkinson and di Girolamo (2021) mention an overall slight increase (until 2014), the latter also shows the clear increasing trend in the austral summer months (Figure 2 (a) and (b) of that paper). However, we do not think that we need to include all this in the manuscript here as we just want to make the point that an observed slight increase of Antarctic sea ice extent might imply an increase in MYI extent, at least until 2014. We now additionally refer to Parkinson and di Girolamo (2021), now L43

(8.) L53/54: "For these reasons ... two regions" --> I don't agree to put this sentence here because you have not yet made any link between the physical properties that you just described and how these influence radiometric and backscattering observations.

We think it can be taken for granted that the structure of sea ice has an influence on its radiometric and scattering properties, even without going into detail here. We have replaced „are different“ by „are expected to be different“ (now L57).

(9.) L62: Please provide at least one reference each for these two important implications of landfast sea ice in the Antarctic.

It was the reviewer who gave us the information about landfast ice here (without references) and requested to mention it, which we had agreed to. We have added a recent reference on that (Fraser et al., 2021), now L65.

(10.a.) L76/77: "exclusively to the Arctic" --> Why is this the case? Because the radiometric signature between MYI and seasonal ice in the Antarctic was found to be not well suited to carry out a similar analysis in the Antarctic - at least when using the same channels as in the Arctic.

We have the impression the reviewer would like us to insert this explanation into the text, which we readily do as it adds clarity (now L79-81)

(10.b.)- "Besides, the retrieved ... see Section 3.2" --> This is a too global statement. First of all it needs to be stated whether you refer to MYI extent or area. It is the area which cannot increase during winter while the extent may increase during periods of MYI fracturing and dispersion during divergent conditions - depending on the threshold set to compute extent and depending on the capabilities of whatever algorithm used to reliably discriminate MYI from other ice types. Secondly, this tendency of MYI whatever to increase during the cold season is something that may occur but that not occurs regularly (I know this from own experience working with the NASA-Team MYI concentration data). Hence, I suggest to rephrase your statement.

We have specified that we mean the MYI *area* (now L81). Our wording „tends to increase“ actually means that it often, but not always, increases. We do not say „has an increasing tendency“ which would mean it always increases which is not the case as the reviewer pointed out.

(11.) L124/125: "the radiometric ... or even vanish" --> Since you are referring to Arctic conditions here it would be suitable to cite respective papers dealing with sea ice type retrieval in the Arctic, for instance the two Lindell and Long papers from 2016 in Transactions of Geoscience and Remote Sensing, 54, or Remote Sensing, 8.

We now cite one of the papers (the one that includes ASCAT data) – now L132.

(12.) L175/176: "but is FYI ... of the ice" --> I suggest to add that this can also be FYI with a thick snow cover known to resemble radiometric properties similar to MYI ice (as is one of the problems using the 37 GHz / 19 GHz vertical polarization gradient ratio for snow thickness retrieval).

We have added „or because of a thick snow layer“ – now L184/185

(13.) L179: "non-MYI" --> You write that you keep this new pseudo-ice type which is either FYI or YI - so simply seasonal sea ice. But I cannot see it in any of the maps you show later. To which of the concentrations is the concentration of this non-MYI added? If it is not added anywhere ... where does it remain? Or is this the "magic" missing 20% in some of the maps of areas with 100% sea ice concentration?

We admit one could do more with the non-MYI. If we just had two ice types, MYI and seasonal ice, we could simply add the non-MYI to the seasonal ice. However, as stated in the manuscript, (in the same line), we cannot tell how of it is FYI and how much is YI, so we just keep it and do not use it. One approach , e.g., would be to split the ex-MYI into FYI and YI according to the FYI and YI retrieved in that grid cell. We have inserted part of this discussion in a footnote that refers to what is now L188.

Speaking of "magic missing 20%" is imprecise and sounds even slightly polemic- it would have been good to stick to the facts instead: the total ice concentration from ECICE is between 80% and 100% in some areas where the SoD charts show 10 tenths of ice cover, in particular in areas of YI and FYI in the inner Weddell Sea (cf. item (31.)).

(14.) L204/205: "This is the common grid used ..." --> I don't think that the information who uses this grid is required. Widely used as well is the EASE grid which has several advantages over the polarstereographic grid and is used, e.g., by OSI SAF for its sea ice concentration products. What is required, however, is to mention the latitude of the tangential plane of that polarstereographic grid.

We have mentioned the NSIDC just because the grid resulting from this specific polar stereographic projection with corners at at 39.23°S /42.24°W, 39.23°S /42.24°E, 41.45°S/135°E, and 41.45°S/135°W, standard longitude of 0° and standard latitude (latitude of the tangential plane) of 70°S, is very often just called „NSIDC grid", and it is still widely used, even though other, better grid projections (EASE grid) are increasingly being used. We did not want to put all these details here as they are not essential for understanding the paper and would make it clumsy, but instead refer to our data user guide. Note that the effect of different standard latitudes is just a different scaling of the whole grid, so the standard latitude for the „NSIDC grid" it is not needed at all to understand the maps. The standard latitude is essential, of course, to correctly interpret the meaning of „nominal grid resolution". So when a reader wants to actually use the data (e.g., our data), the accompanying documentation gives all the details needed.

(15.) L210/211: "using a simple linear approach ..." --> Did you develop this by yourself? In that case it would be appreciated to learn more about the method and its application frequency (daily? monthly? seasonally? How often are the linear regression coefficients updated? What is the data set used to define where there is sea ice and where there is open water? Is a sea ice concentration threshold used? What about summer conditions?). In case not please provide an appropriate reference.

This approach was developed in the group of C. Melsheimer and G. Spreen, based on the observation that the dependence of the NRCS (in dB) of sea ice on the incidence angle for ASCAT can be well fitted by a straight line (also observed for ERS Scatterometer by Gohin &Cavanié, 1994, DOI:10.1080/01431169408954156). The data were taken over regions with near 100% ice cover in January 2016 in the Arctic, and the slope p determined from the data is used to convert NRSC $s(t)$ from incidence angle t to 40° according to: $s(40°)=s(t) + p*(40-t)$. While this is a rather simple method, it works well: daily composites of ASCAT NRCS data thus converted have not shown any visible swaths. We have included this information in the new Appendix C.

(16.) L214-217: "For FYI and MYI ... per day" --> These lines and your comments to my 1st review do not lay out sufficiently well what you did why. Please put yourself into the position of a student who wants to redo your analysis ... The following points need clarification:

(16.a) Why did you choose 2018 as the "master" year for your initial discrimination of sea ice types to find the probability distributions of the parameters laid out in Table 1?

We started adaptation of the algorithm to the Antarctic in 2017/2018, so we chose rather recent data and ended up with 2018 (now mentioned in L231). We admit this is somewhat arbitrary. We have also improved the explanation why we have chosen this specific approach for FYI and MYI (now L227ff.).

(16.b) While you attempt to describe better in your footnote 2 how you define what MYI is, it is not sufficient. First of all, the moment when the sea ice cover starts to grow again, resulting in an increase of the sea ice extent depends pretty much on the drift conditions. I can envision - and this is also an issue in publications where melt and freeze onset are derived - that on a day-to-day basis you cannot adequately define when sea ice indeed begins to advance again. Those publications / methods use - for good reason - a period of 3 or even 5 consecutive days of the respective geophysical process to happen (in your case sea-ice advance) to define that the conditions have switched from summer melt to freeze-up. Secondly, you do that "regionally" ... okay ... What are the regions? How did you define them? How large is the difference between freeze-up (or ice-advance and hence MYI definition) days between different regions? I can envision that MYI is defined as such as early as mid / end of February in the Weddell Sea but as late as May in parts of the Bellingshausen Sea. Here, your manuscript lacks essential information.

Note that the approach to look for the sea ice to start grow again is just used to identify MYI sampling areas. This is not an analysis about where in the whole Antarctic the cold season starts on which specific day. We are aware of the complexity of the topic. Here we only want to deal with identifying a handful of regions (see Table A2) of ice that is left over from the previous season and make sure that refreeze has started. By observing daily maps over a few weeks from February to April it is very well possible to distinguish just drift from real ice growth because only in the first case the concentration would go down if the extent increases. We have moved the text from the footnote into the text and elaborated more on this: „For this purpose, we have observed the daily ASI sea ice concentration maps in February and March […] before possible drift blurs the picture, the areas of MYI can be identified.“ – now L232-237. We admit that there might have been a more sophisticated way and will address that, along with the general weaknesses of identification of sample areas, later in the discussion (end of section 3.2, L436-456).

We also have to address another possible misunderstanding: We do not define our "initial" MYI as all remaining ice at a certain day, or regionally at certain days. We always take the MYI that is output by ECICE, which is now clearly stated towards the end of section 2.3, L290ff.
However, the correction for snow metamorphosis, deformation, snow accumulation(i.e., processes that make non-MYI look like MYI), called drift correction, needs a reference MYI domain from the previous day. On the day which we use as starting day and which should be during the beginning of freeze-up, we use the (uncorrected) MYI from ECICE from the previous day for that. After that, we always use the corrected MYI form the previous day, of course. Note also that the mentioned processes that make FYI look like MYI permanently can be expected to be still weak in the first few weeks of the freezing season, therefore, the corresponding correction (the drift correction) is not very important at that time. This means the result (the corrected MYI concentration) should not depend strongly on the beginning date for the drift correction. We have included this information in the manuscript, in section 2.2.2 “Drift correction” (now L187ff).

(16.c) "Later in the season ..." --> I probably understand what you did but it is not described properly. When sea ice is defined as FYI becomes not clear. Do you apply a sea-ice motion data set to actually track the border of the MYI defined in the previous step? Or did you apply a ball-park estimate of 10 km drift per day? In the latter case the question would be: i) into which direction and ii) every day?
This is an estimate of the drift, just to get a approximate upper limit., no sophisticated tracking involved. We have added a word to stress that this is just approximate (now L240).

(17.) L223/224: Did you check the weather conditions in the regions and during the time periods chosen to take your open water samples? Are these from predominantly quiet conditions (low wind speed, low atmospheric water vapor load, low cloud cover) or do these cover a wide (an as wide as possible) range of different conditions and hence atmospheric influences on the AMW and PMW observations? It would be useful to provide this information in the context of showing and describing the respective distribution functions.
As can be seen from the size of the OW sampling areas (about (600 km)² and (250 km)², respectively) as well as the time period (5 days, 4 days), they will definitely include widely diverse atmospheric and oceanic conditions. We have added a sentence stating this, now L247-249.

(18.) L224/225: "Details of all ..." --> Thank you for providing this information at least in form of a table. For the understanding and the credibility of your proof-of-concept study it would be much more useful, though, to have a map with the sea-ice concentration of, say, the first week of March 2018 (noting that several of your samples are dated around this period) superposed with the lat/lon boxes from where you obtained the data for the FYI and MYI samples. You could use such a map also to illustrate the regions that you apparently used to define freeze-up day and hence the date from which onwards a grid cell is defined MYI.
We do not want to overload the paper with illustrations and think it is credible enough to describe how we selected these areas, on which we have elaborated now, as stated in our detailed response to item (16.2) and the corresponding extra information in the manuscript. Note that we have not defined (and need not define) a regional „freeze-up day“, see item (16.2) as well.

(18.a) - I note that YI samples are chosen across the entire freezing season and also FYI samples originate from three different time periods, increasing the likelihood to cover the potential range of surface conditions. Only for MYI you restricted the samples to March which I believe is sub-optimal. Particularly in the Weddell Sea Sentinel-1 SAR imagery allows to clearly separate MYI from FYI also later in the season (just take a look at the SAR images provided regularly by the Polarview consortium). This is a lack of rigorosity in the selection process of the MYI samples. I note in addition that you have substantially fewer MYI samples (about 1000) than other samples (FYI about 6000, OW about 14 000, YI about 2000). Given the fact that in the interpretation of your results you focus quite a bit on the MYI cover distribution this seems to be a sub-optimal and not sufficiently well-thought through way to generate a data set of samples to be used. MYI signatures can vary a lot during winter and particularly in March they might not be representative for conditions met later at all

Admittedly this is not ideal, but there is a reason for the seeming “lack of rigorosity”: The problem is to find reliable MYI without using other data sources (which was our guideline). We see now that external data to identify MYI areas throughout winter might really improve the retrieval. However, it is not trivial to identify large enough areas of, if possible, pure MYI (100% MYI concentration). The sample area selection is now described in more detail (see items (16.a) and (16.b)), and further discussed towards the end of section 3.2, L436ff.

(19.) L233/234: I am glad that you put your observations into the context of values published in the literature. I am wondering however, whether you are fine with the fact that for FYI the GR3719 is well below zero ... which is indicative of what? Figuring this out will aid you in your interpretation / discussion of your results which is not yet convincing.

For FYI, GR_{37V19V} is close to zero for bare ice surface and becomes progressively negative as the snow depth increases. In other words, GR_{37V19V} is a function of snow depth on FYI, and used as such in a few algorithms to estimate snow depth. That is why the use of the range of this parameter for FYI in ECICE is useful (as opposed to using a single value). The use of the range of GR_{37V19V} does not help in interpretation of the results but it helps in producing more accurate classification of ice types.

(20.) L234-238: "The MYI tie points ... distributions." --> Your "well above" is something like 5 - 8 K which is not that impressive. Your observation that the modes of your MYI TB distributions do not match well with MYI tie points in the published literature could very likely be caused by the fact that you selected cases from March - a time of the freezing season where the MYI signature might not be representative of winter conditions. You could take this observation as a hint that your cases to derive MYI TB and sigma_0 samples are not well chosen.

We have changed the text to „about 5 to 10 K above“ (now L260). We now mention that our samples are from the beginning of the freezing season and might thus not represent the whole range (L262-264).

(20.a)- The comment you make in the last sentence does not really help here since Arctic MYI differs substantially in its physical and hence microwave structure from Antarctic MYI ice - simply because the melting process differs fundamentally. I therefore suggest to delete that last sentence.

We agree. Deleted.

(21.) L238/239: You state that your C-Band NRCS values for Antarctic MYI are higher compared to what is observed in the Arctic. Did you check with results from the Antarctic? Haas (Annals of Glaciology, 33, 2001) reported C-Band NRCS values of Antarctic perennial sea ice of -16.3 dB compared to about -10.7 dB during summer. Gohin (International J. Remote Sensing, 16(11), 1995) reported winter-time NRCS values of MYI of -14 dB. Finally, looking into Arndt and Haas (The Cryosphere, 13(7), 2019) I find C-Band NRCS value time series for FYI and MYI. In short: There is a lot of literature you could have taken a look into to illustrate how well (or not well) your MYI NRCS values agree with published literature in the Antarctic; there is no need to look into the Arctic

We have removed comparison with Arctic data and replaces this by comparison with results by Arndt & Haas (2019), and we also point out that according to the time series in that paper (Fig. 4a) the highest MYI backscatter values occur in autumn and early winter which is the time from which our MYI samples stem.

(22.) L242: "the only source ..." --> How about NCEP/NCAR, MERRA-2, or JRA-55 re-analysis data? Are these not comprehensive and consistent as well?

We agree, the wording was not good. We of course mean that meteorological reanalysis data are the only source of comprehensive and consistent temperature data over the Antarctic sea ice, and we have modified the text accordingly, now L268-270.

(23.) L248: "Sea ice drift data ..." --> why is that? Please provide a reference which backs up this notion.

Lavergne et al. (2021, DOI:10.5194/tc-15-3681-2021) found larger standard deviation in the Southern hemisphere when comparing ice drift from PMW with buoy data than they found for the Northern hemisphere (Table 2 and Fig. 5 of that paper). Possible causes: (1) larger drift speed in the Antarctic, so the motion tracking algorithms have to cover more area before they find the maximum cross-correlation which leaves more room for ambiguities in the motion field; (2) the processes that modify the ice in the Antarctic are often stronger than in the Arctic, hence the radiometric signature of the ice is more unstable which also hinders the tracking algorithms; (3) Antarctic sea ice is in lower latitudes than Arctic sea ice, so there is less overlap of the satellite swaths and, hence, less abundant coverage. Added the reference and a short remark (L275-276).

(24.) Figure 2: The color in the legend does not match with the description given in the caption and in the text. This has to be corrected. I guess you switched YI and MYI.
Yes, indeed! Thanks for finding this error which sneaked in when modifying the figure for the first revision. – Corrected.

(25.) - Please refer to appendix A and the relevant tables of the regions used to define these distributions and also provide the total sample numbers of the four different surface types shown.
We now refer to appendix A in the caption of Figure 2 as well, and also specify the total number of samples for all four surface types.

(26.) L285/286: "and the ice is more porous because ... scattering." --> Please cross-check this statement with the paper by Haas and Arndt and Haas I mentioned further up in this 2nd review of your paper.
We just want to give two possible reasons for the fact that in SAR images, MYI looks brighter than FYI, i.e., the NRCS of MYI is higher than that of FYI – we have added "e.g.," to make this clear (now L316).

(27.) L286: "we manually scaled ... classes." --> just a comment, no action required: This is another element of your work / paper which is not sufficiently transparent such that an interested student can redo the analysis the same way as you did it.

(28.) L287: "19 Sentinel-1 scenes" --> "Sentinel-1 SAR scenes"
Corrected.

(28.a)- You must provide the source of this data.
Added data source (https://scihub.copernicus.eu).

(28.b)- You write "in the Weddell Sea and near the Antarctic Peninsula". This applies to me that you used SAR images covering the Weddell Sea but also parts of the Bellingshausen Sea? ... As stated later on, a map would solve this lack of information (or ambiguity of the information provided).
We have added overview maps for the two days and included them in Figure 3.

(29.) Figure 3, L296/297: "Qualitatively, the ... is similarly good." --> What keeps you from showing a map into which you plot all 19 SAR images and superpose the ice type isolines as you did in Figure 3? One could get the impression that you picked the best example here and that in fact the agreement is less (even qualitatively) good for the remaining 18 images - simply because these might not focus that well on the wonderful MYI-FYI boundary shown in Figure 3. I know, it is just a proof-of-concept study and for this I do not expect a quantitative analysis of the SAR images used anymore and I do not expect something like a quantification of areas of misclassification (or good classification). But what I can expect is that you provide the reader with a sufficient amount of information that allows the reader to believe in the credibility of your results. In your case this means: show all SAR images (If you are in doubt of creating too many figures then --> merge Figs. 13 to 15 into one figure).
See above, we have added overview maps for the SAR scenes now.

(30.) L305-307: However, no ..." --> Just a comment: This makes a lot of sense because there are places around the Antarctic coastline where sea ice advance kicks in as late as July.

(31.) Figure 6 / L322-324: I commented on that in my 1st review and do not see it solved here: The color scale used in Figure 6 implies that the total sea ice concentration for the majority of the areas downstream of the Filchner-Roenne Ice Shelf is less than 100%; the color suggests something between 80% and 90% with higher FYI concentrations occurring actually rather at the boundaries of the FYI areas than within the areas themselves. Therefore, please tell the reader that the partial concentrations shown in Figure 6 do not need to sum up to 100% (even though I don't understand why.). Otherwise the reader sees that there are areas where MYI concentration = 0%, YI concentration = 0% and FYI concentration = 80% in regions where the SoD maps clearly indicate 100% and would at least have doubts about your product. It is pretty clear that in these areas OW concentration cannot be 20% (and if so, then your algorithm is not producing correct results here).
From the first review we had the impression the main problem was that we had failed to explain clearly enough that the partial ice concentrations of YI, FYI and MYI do *not* add up to 100% but only to the total ice concentration which can of course be less than 100%. We have made some effort to explain this clearly in the revised manuscript (several places) and also in our response to the review (notably, in our response to GC4, and in items 26.6, 28.4 and 30.4. of the first revision). Upon rereading the first review, we notice that another concern of the reviewer was that the sum of the partial concentrations seems to be 80% to 90%, while the SoD charts show 10 tenths of ice cover (they only indicate tenths, not per cent, which makes a difference as

to the precision meant). However, noticing that the AARI and NIC charts of the same day, 30 Mar 2017, shown in Figure 5, show 8 to10 tenths and 10 tenths. respectively, for the area in question, we did not think 90% total ice concentration to be a problem. Indeed, the total ice from ECICE shows indeed 80% to 95% (ASI maps show about 90%). We briefly discuss this issue now, L374-379.

(32.) L330/331: "The FYI areas ..." to 70%" --> I am not agreeing completely here because particularly in those regions where we have these strips FYI concentrations are often as high as 100%.
Inspecting the figure again, we do not see 100% FYI concentration where there are strips of about 20 % to 40% MYI concentration. However, the MYI concentration is sometimes as low at 20% in the MYI strips of the SoD charts and the strips are of course not fully congruent. This is not surprising as the SoD charts are weekly charts, the retrieved ice type maps are daily averages. We have modified the text in this sense, now L361-364.

(33.) L354: "no areas of more than 40% YI concentration ..." --> Since you refer to YI concentrations in the northeastern part of the region shown in the previous example I suggest to detail this statement a bit better here.
We agree, so we have added:“, with the exception of some narrow areas on the coast/shelf in the Northeastern part (top right) of the box“ (now L395)

(34.) L359-361: Is this rather global statement at the beginning of this short paragraph relevant and correct? I would say, how large icebergs are classified depends on whether their radiometric signature resembles that of MYI or any of the other ice types and this signature changes with the season. Your results do not support that icebergs are generally classified as MYI (compare Fig. 3 and 4 as well as 6).
We agree and have changed the text accordingly (now L398-399)

(35.) L364: "increase around July" --> In view of Fig. 11 this is clearly an underestimation of the situation. If one would compute a running 30-day mean of the time series shown in Figure 11 one would get an upswing of the MYI area that begins in May/June and ends in August before again an increase in MYI area kicks in during October. I recommend to state the situation in a light that is more in line with what the figures shows.
We write “the total MYI area shows [...]in most years even an increase around July” – this is, of course, not a description of Fig. 11 (year 2018) but a succinct summary statement for all investigated years. We have now added a few words mentioning that in the 2018, shown in Fig. 11, the increase is earlier (now L405/406). Apart from that, we think it is clear that Fig. 11 is meant to underline our observation of large fluctuations as well as an increase around July in most years, and do not want or have to discuss all details of the Figure.

(35.a)- Did you check the minimum sea ice area at the end of summer in Feb/March 2018? Is your initial MYI area in line with the minimum sea ice area?
According to NSIDC, the average Antarctic sea ice area for February 2018 was about1.6 Million km^2, the ASI data record shows a mininum of about 1.5 Million km^2, which is higher than the start of the MYI curve in Fig. 14. However, as mentioned already, we use the MYI extent retrieved with ECICE, and in late February, not all ice is retrieved as MYI: there are coastal polynyas that that are not MYI, and some ice in the southeastern Weddell Sea is (still?) retrieved as FYI (also visible on the NIC SoD chart: http://ice.aari.aq/antice/2018/02/20180222_nic/nic_antice_20180222_sd.png).

(35.b)- Before you begin with explanations what might cause this increase during winter I recommend to first briefly explain how you expect this curve should look. I am wondering in this context whether the intial decrease of the MYI area during March and April is not far too large and/or rapid.
We have stated one line above that we expect the curve *not to rise*. We think we cannot be more specific.

(35.c)- Finally, a piece of information that is (still) clearly missing is how accurate and credible your intitial definition of MYI is. You did not come up with this and your footnote 2 a few pages up is not sufficient to understand how you regionally defined the starting point for your MYI occurrence. From what you wrote I can only speculate that the dates from which onwards ice present in a particular region is classified as MYI differs between the regions and that this could have had an impact on how Fig. 11 looks.
As stated above (item (16.b)), we do not define all remaining sea ice at a certain day (or in different regions on different days) as MYI. We always use the output MYI of ECICE. The start of the freezing season is needed in order to have a starting point of the correction for FYI and YI that looks like MYI but cannot have drifted there (drift correction). As discussed above, the result is not strongly dependent on the exact day.

(36.) L377/378: "Usually, snow ..." --> Please be more specific about the processes that can occur during winter on sea ice relatively close to the ice edge. It is deformation, increasing roughness. It is snow thickness increase due to snow accumulation, resulting in a decrease of the GR3719 towards values that resemble MYI (but I note that your FYI samples exhibit a quite negative GR3719 anyways ... unlike in the Arctic, see your Figure 2). It is snow metamorphism due to air-mass change induced melt-refreeze cycles. It is flooding of the ice-snow interface with formation of slush at the basal snow layer which eventually refreezes to become snow-ice - an ice type widespread in the Antarctic which radiometric properties not well studied.

We think that our wording "Usually, snow backscatter increases with time and emissivity decreases, making it resemble MYI in that respect" is an appropriate summary statement that gives a reason for spurious MYI and hints at possible problems with the distributions.

(37.) Figure 12: Also for these maps you must detail why the sum of the ice type concentrations does not need to sum up to 100% but can have any arbitrary value between - say 80% and 110 or even 120% - for areas with 100% sea ice concentration.

No, the sum of the ice type concentration cannot have „any arbitrary value between […] 80% and […] 120%". The reviewer seems to have misunderstood our explanations in L137 and L334 of revision 1 of the manuscript and our responses to several items in the first review (see also item 31. above) that the sum of the partial ice concentrations of YI, FYI and MYI *plus the open water fraction* is 100%. Thus, a MYI concentration of, say, 50%, does not mean that of 50% of the ice in that grid cell is MYI, but that a fraction of 50% of *the total area of that grid* cell is MYI. And, of course, the sum the YI, FYI and MYI concentrations can have values between 0% and 100% as it is just the total ice concentration.

Note that only in case the corrected MYI concentration is higher than the uncorrected one (which can only be caused by the temperature correction), the sum of YI, FYI and corrected MYI concentration might in principle be above 100% , but only if the total ice concentration is close enough to 100% that the increment by the temperature correction makes the sum go over 100%. Also note that the temperature correction is episodically and local and usually restricted to autumn.

(38.) L379/380: What about the emissivity of pancake ice? Backscatter is just one out of the four input parameters.

In the distributions of the other input parameters, there is overlap between MYI and the other surface types, but the MYI backscatter (NRCS) distribution stands apart from those of all other surface types. Therefore, it is increased *backscatter* by pancake ice that is most likely to cause confusion. This is not meant as an exact analysis of the scattering and emission behaviour of ice types and subtypes, but about assumptions what could be insufficient in our sampling area selection (see response to GC3 above).

(38.a)- Also, I note from your sample maps of the YI distribution in Fig. 4 but also here in Fig. 12, that there is a fringe of elevated YI concentrations along the ice egde being indicative of the presence of pancake ice. It won't be any of the typical sheet ice types such as nilas or grey ice - these ice types are only interspersed in pocket like structures in the otherwise dominant pancake ice cover (see e.g. Ozsy-Cicek et al. 2011, Annals of Glaciol). Hence I would guess that your approach deals reasonably well with pancake ice.

Yes, it might be that our approach deals reasonably well with pancake ice, but this would come as a surprise: Unless there was a substantial portion of pancake ice in the coastal polynyas we sampled for the YI parameter distributions, these distributions do not contain pancake ice. Therefore it is appropriate to keep pancake ice as a concern here.

(39.) L393: The third reason can simply be that your correction approaches do not work the same way as they work in the Arctic because the processes that change sea ice and snow physical and hence microwave properties are more vigorous and different from those in the Arctic. Also their impacts could either be lasting longer or, what is possibly more probably, their frequency is too high to capture them properly with your correction method.

Why should more frequent events not be captured with the correction schemes? We have added a statement that there might be changes to the sea ice in the Antarctic that cannot be corrected by the two correction schemes (L434/435).

(40.) L397/398: "The fact that ... polynyas" --> see my comment above about that your YI concentration maps seem to resemble the pancake ice areas well.

See item (38.a)

(41.) L400-407: I doubt that in the weekly SoD charts of September and October there is an oscillation of areas classified as FYI or MYI in the Antarctic. If so, then you should perhaps consider not to use the SoD charts at all. I have doubts about this oscillation because these SoD charts are always analysed taking into account the distribution of SoD from the previous week, including knowledge about the past ice conditions and their persistence. In a way, your statement also misaligns with your earlier notion that SoD charts are developed by well-trained, well-experiences analysts.
We have not expressed clearly enough what me meant: In general, the weekly ice charts are produced alternately by NIC and AARI, and in addition occasionally they independently produce maps for the same week. There seems to be a slight bias between them as to distinguishing WMO types 2.5 and 2.6. This hints at how similar these ice types appear even to a trained human analyst. We would not, however, as suggested by the reviewer, draw the conclusion not to use the SoD charts at all, in particular as there are virtually no other sources of ice type information for the whole Antarctic. We have tried to improve the text, now L450-455.

(41.a)- Both, September and October are months of freezing conditions. Melt conditions occur, if at all, still episodic and in association with warm air advection ahead of low pressure systems - at it does all year / winter. The same applies to some extent to November. In my eyes, the most likely reason for the misclassification is that your MYI cases used to train ECICE are from March only and very likely do not reflect the deeper, more layered snow pack conditions encountered in September / October - the months where also the NASA-Team algorithm has severe difficulties to correctly retrieve the sea ice concentration of Antarctic sea ice - because of the layering of the snow (see Comiso et al., 1997 in Remote Sensing of Environment, a paper that initiated the development of the NT2 algorithm to get rid of the substantial biases). In addition to the samples used for MYI you might also consider that the FYI samples might not represent the deeper snow pack conditions often encountered in late winter / spring - aka September / October.
Yes, the restricted sampling of MYI at the start of the season is a main concern and is being addressed now – (see items (16.b) and (18.a).)

(41.b)- The strong decrease in MYI area shown in Fig. 11 during September / October could also simply be the rebound to "normal" conditions with all the artificial MYI area discussed in the context of Fig. 12 vanishing. What puzzles me more is the increase in MYI towards spring, i.e. October / November.
We agree that the part of the decline is the rebound when the erroneous MYI vanishes (but why does it vanish?). However, the strong decline continues into October, and the SoD charts do not show a substantial decay of the remaining MYI in that time, which is why we assume misclassification.

According to the OSISAF team (see the ATBD and the validation report of their sea ice type product, Aaboe et al., 2021a and b, reference list of the manuscript), the variability of their retrieved MYI in the Antarctic in September and October is already so large that they classify the September and October data as unreliable. Note that they define "variability" as the standard deviation of the different of daily extent minus an 11-day moving average.

We doubt that the total MYI extent in November is meaningful. In the Arctic, the MYI retrieval becomes unreliable in late May, so it is quite likely that this happens in the Antarctic already in early November, considering that much of the Antarctic sea ice is at lower latitudes than its Arctic counterpart.

(41.c)- In conclusion, I strongly recommend to revisit your strategy to choose sampling location and their time periods, take a look into publications that describe what we know about the physical processes of Antarctic sea ice and snow during the course of winter and how these influence AMW and PMW properties of the sea ice; I recommend to again read Voss et al., 2003; Arndt et al., 2016; Haas et al 2001; Arndt and Haas, 2019; Willmes et al., 2014, Willmes et al. 2011, and others and based on what you learn from studying these publications critically discuss what might be wrong in ECICE as used in your manuscript. As you stated 1-2 paragraphs further up in the context of Fig. 12: It is not the correction which partly jeopardize your results but the complex nature of the Antarctic sea ice and its snow cover that is not adequately taken into account into "tuning" ECICE for Antarctic conditions by simply using local TB and NRCS distributions.
As already mentioned (items (16.a), (16.b), (18.a)), we have put more emphasis now on the selection of sample areas. Note also (see our response to GC3 above) that this is not about finding "what is wrong in ECICE". The problems are the samples for the needed distributions, and possible problems with the correction schemes. in particular the drift correction.

(42.) L418: "A first check is ... total sea ice" --> Certainly this is a good idea - however, the examples you present in your manuscript do not credibly support such an approach since - at least from visiual inspection of the maps shown - even for 100% sea ice cover the ice type fractions do not add up to a total sea ice concentration of 100%. This needs to be clarified better / corrected before you can consider to carry out the step described in this line.
Firstly, with "100% total ice concentration", does the reviewer mean the 10 tenths of total ice concentration on the SoD charts? As mentioned above, there is some uncertainty in the ice cover information from the SoD charts, so this should not be overrated.
Secondly, note that when comparing time series of some geophysical variable such as sea ice area or concentration from different data sources/algorithms, systematic differences are almost inevitable (think of the ASI sea ice extent time series versus the NSIDC one), but the main thing is the anomaly, or, in other words, the time evolution and the ranking of maxima and minima (see, e.g., Nature 478(7368):188) usually contains the main message.

(43.) L421/422: I don't understand what you mean by "as the standard one" and it certainly does not surprize you that I suggest to add to the plot at least one time series of a sea-ice concentration product based sea ice extent, e.g. OSISAF (not the one from Tonboe et al., 2016 but the newer one from Lavergne et al., 2019, i.e. OSI-450 / OSI-430-b).
We have introduced our meaning of „standard“ two sentences before. Changed to „as the ``standard" ones (here: ASI)“ – now L472.

(44.) L425: You sum up these areas using all non-zero concentrations, am I correct? What I mean by this is that you do not use a 15% threshold.
Yes, of course, as we calculate the area here. We have inserted „0% to 100%“ in the parenthesis for extra clarification (now L478).

(45.) L429: "mainly caused by ..." --> I suggest to re-formulate this sentence because it is very evident in your manuscript that this correction is most likely not the main reason but the fact that your choice of sampling areas and periods for MYI but potentially also for FYI is not representative enough for the full range of conditions encountered during the course of the winter. I suggest not to blame the correction at first place.
Yes, we have reformulated the sentence, now L481-483.

(46.) L431/432 / Fig. 15 (and also Fig. 14): What is the minimum sea-ice area of the respective years?
As mentioned in item (42.), we consider the ranking of the minima and maxima more important than the absolute values.

- A "." is missing at the end of the sentence. Corrected.

(47.) L452/453: "The data become unstable ... underestimated." --> Why is that? Shouldn't the MYI area in September / October be smaller than the MYI area you begin with in March? I'd say that a substantial amount of the MYI found in the Weddell Sea end of summer has melted by then in the Southern Ocean facing the Atlantic and should continue to melt in November. Hence the low September / October MYI area does not seem unrealistic to me but the increase in MYI in November does.
See item (41.b)

(48.) L459-462: May I propose a slightly corrected version of this? For (1) I clearly see the need to incorporate also again MYI into the revision of the distributions. Step (2) should be a thorough evaluation of these TB and NRCS distributions with values available in the literature. Step (3) should be a quantitative evaluation of the ECICE results before any correction with a considerably enhanced and better analyzed set of evaluation data which may or may not contain in-situ and ship data (I would say the latter are most useful for the identification of pancake ice locations). Then, step (4) could be an application of the corrections with more up-to-date data. Step (5) would be a final evaluation. Only step (6) can then be an extension of the current data product using AMSR-E and respective AMW sensors plus - again - an evaluation of the extended product.
We thank the reviewer for the suggestions and have included MYI into the revision of the distribution , and slightly modified the other points as well (now L512-517)

Typos / Editoral comments:

(if not otherwise noted, the correction was done as suggested)

L43/44: "In the absense of ... still unknown" --> Are you referring to different surface topography and hence impulse fluxes or to different thicknesses of sea ice and its snow cover and hence heat fluxes?
We wrote “energy fluxes” and had mainly the heat flux in mind. Changed to “heat flux” (now L45/46)

L47: "and thus" --> "and the ice-snow interface thus"
And further: "... flooded. The slush layer resulting from flooding of the basal snow layer with sea water eventually refreezes, becoming snow ice."

L50/51: "to higher wind that triggers" --> "to more variable wind conditions that cause"

L98-103: Please stress more that the following two sections, 2.1 and 2.2, are valid for the ECICE as developed in the Arctic - including the two correction schemes.
We have inserted the note „originally developed for the Arctic“ in L108/107 and L108. The ensuing description of ECICE (section 2.1) and the correction schemes (section 2.2.), however, is a description of the general algorithm and not just valid for the Arctic. They become region-specific as soon as ECICE is given the distribution functions and the correction schemes get their tuning parameters.

L302: "and reconnaissance" --> is this air reconnaissance? Then please add this important detail.
We gave checked the documentation of NIC on how the charts are produced (https://usicecenter.gov/Resources/AnalystProcedures) and actually found no mention of reconnaissance data. However, we found no information for the procedure at AARI (neither on the English nor the Russian web pages), so we delete the mentioning of „reconnaissance“, but insert the above link to the NIC procedure .

L312: "plus correction schemes" can be deleted as you write "corrected MYI concentration"
When we delete this, the statement would read „corrected MYI concentration from ECICE“ which is wrong as ECICE does not output corrected MYI concentration. Therefore, the „plus correction scheme“ cannot be deleted.

L332: "one sea ice type class" --> add "such as in the SoD charts"

L333: "retrieval where" --> "retrieval such as ECICE where"

L358: "given the limitation ... yet." --> I suggest to delete this part of the sentence; it does not add relevant information.

L364: "form" --> "from" –

Figure 13: You can remove the title. Both, the y-axis title as well as the legend state what is shown.
A "." is missing at the end of the caption.

Figure 14: As in Fig. 13 you can remove the title.

- You could also plot the total area and compare it with the total area derived from the OSI-450 / OSI-430-b product. This would lend the figure more credibility.

Figure 15: The figure is cropped at the left side.
Noted. Will be fixed for final version.

Like in the previous two figures the title can be removed.

L434: What is "maxima of total ice" --> please be more specific in your wording. What you mean are "maxima of the total sea ice concentration".

L455: "i" --> "in"

L457-459: This sentence should be merged with the next paragraph with (currently) point (4).

L468/469: One mentioning of "at 1.4 GHz" seems enough here.