Response to RC1:

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

General Comments

Here is our response to the four general comments (GC1 to GC4)

GC1: Physics

It is true that sea ice physics is different between the two polar regions. We agree with the reviewer on the 3 listed aspects, namely the MYI age and formation, the dynamic environment of the sea ice and the differences in the snow depth and composition. There are more aspects manifested in ice motion patterns, ice kinematics (particularly lead formation) and dynamics of marginal ice zones. However, characterization of those differences is beyond the subject of this study. Differences in ice physics between the 2 regions warrants a separate study, perhaps in the form of a review paper or a book chapter. In this regard, we would like to assert that little is known about sea ice physics in the Antarctic compared to the Arctic. Just a quick look at the literature is enough to prove this point. Most books on Arctic sea ice have an opening chapter on ice physics. This is not the case with the only book on Antarctic sea ice (published in 1998, edited by M Jeffries). Nevertheless, a summary of the major differences between gross properties of sea ice and snow in both polar regimes can be compiled and included in the Introduction section though this would increase the length of the manuscript, which we are hesitant to offer. Instead we have slightly extended the section dealing with the differences between Arctic and Antarctic sea ice (L43-50 of original manuscript) and added references (see item (2.) and (3.) below.

The objective of the study is stated as to utilize the ECICE algorithm to quantify the concentration of each ice type (YI, FYI and MYI) with a focus on MYI in the Antarctic region (similar to what has been done for the Arctic ice). It is true that the physics of ice and snow impacts the observed signal (from passive and active microwave sensors in this case) but we do not have to dig in to address the questions of how and why. All what we need to do is to provide the algorithm with a realistic probability distribution of each observation used in the processing for each ice type (without having to explain the physics behind them). This is already covered in Section 2.3. This is no different than the description of other commonly-used sea ice concentration algorithms that use tie points to represent the used radiometric parameter for a given ice type (e.g., NASA Team, Bootstrap, ASI, etc.). In the original papers of those algorithms there is no description of the physics that engender the tie points. Only sampling from homogeneous area of each ice type was needed. This is what we have done in this study (and in the application of ECICE in the Arctic), namely relying on samples from representative area of the given ice types. ECICE has the advantage of using the probability of occurrence of all possible radiometric values from a given ice type, not just a single tie point.

Finally, yes … the snow cover affects the observed signal in ways that we do not fully understand. That is why using the probability of occurrence of all possible values from a certain ice type when covered by snow under different conditions becomes necessary (the advantage of ECICE as mentioned above).

GC2: Previous work

The reviewer has raised a few points under this title. We provide a few more statements in the Introduction to point out the benefits of ice type mapping to the modelling community. However, we see no need to discuss the sea ice type mapping in the Arctic (we already provide reference to the ECICE application to the Arctic ice). Less attention has been paid to the Antarctic sea ice simply because there are no economic or geopolitical benefits attached to it. A case in point is the yet unclear impact of global warming on the Antarctic sea ice, when so much information (observation and modelling) is readily available about the impacts on the Arctic ice.

As for the current knowledge about Antarctic sea ice type distribution, we now provide more information in the Introduction but not about the typical emissivity, brightness temperature and radar backscattering as suggested because we see this as distraction from the objective of the manuscript.

One final point in response to the comments under the GC2 section: ECICE was not modified to apply it to the Antarctic ice. Only the suitable input data had to be used. This is now stated explicitly at the beginnig of section 2.3.

GC3: Description of the methodology

We have completed the description. There is no difference in the application of the method between Arctic and Antarctic. The difference is in the input data (see above). We have slightly extended the explanations about the sampling and have also added an appendix with details about the sampling areas.

GC4: Description and interpretation of the results:

The reviewer raised the point of background information about the difference in physical properties between Arctic and Antarctic sea ice. We have addressed this point in GC1. The reviewer also suggested that it was mandatory to include an expert on Antarctic ice and snow to appropriately interpret the results.

We would like to offer the following arguments. The algorithm is about identifying and quantifying the 3 ice types in each resolution cell of the data based on the input probability density function of the used radiometric values for each ice type. If the input is wrong the output is wrong. Therefore, what is needed from an expert is help to identify authentic samples of a given ice type to construct the distributions. We will check this point, though not raised by the reviewer.

The actual suggestion of the reviewer is to use the knowledge of the expert to support the conclusions from the results. The reviewer presents 3 good themes as examples: the spurious MYI in the Weddell Sea and Ross Sea, the likelihood of leaking YI signature into FYI, and the handling of the iceberg signature, which confuses the identification of MYI. We have thought about including an expert coauthor to help addressing these issues, could not really think of one, besides the effort it would mean for someone to join in the middle of the work.

As for the situations where the sum of the 3 ice types do not sum up to 100%: Note that the (uncorrected) concentrations of the three ice types *plus* the open water fraction add up to 100% (which is guaranteed by the equality constraint in ECICE (see L111-113 of original manuscript) whereas the sum of the three ice type concentration adds up to the total ice concentration which can be 0% to 100%. See also the specific comment on that, item (11.), below.

Finally, yes, in this study we aimed at qualitative evaluation of the results because we thought that this would be appropriate for a first study to apply a new technique to the Antarctic sea ice. The purpose is to “prove the concept” rather than provide a comprehensive data set on ice type distribution for use in models. We nnow state this explicitly at the end of the introduction. We have worked to complete the description of the data used for the evaluation. and try to incorporate some quantitative evaluation in certain areas where more information is readily available in the literature, e.g., MYI in the Weddell and Ross Seas.

Specific comments

Note that we quote the reviewer's specific comments and suggestions in red and have numbered them.

(1.) Line 33-34 "... but it is ... satellite data" is perhaps an a bit too general statement which i) could be specified better by telling the approaches used of doing so (aka: using instantaneous microwave observations) [note: using multi-annual time series of satellite data would work as well], perhaps by including the work of Comiso et al. (2011?) who figured out the differences in the signature of Arctic SYI vs. MYI, and which ii) could be amended by the fact that sea-ice age data retrieved for the Arctic (but not the Antarctic) are based on ice motion data which are in fact derived using satellite data. Hence it IS possible but nobody looked into it yet.

We now say "...using satellite data directly (i.e., not using multi-annual satellite observation or drift data based on multi-temporal satellite imagery)"

(2.)L43-50: This paragraph is meant to provide the fundament for why ECICE needs some form of adaptation when applied to Antarctic sea ice. In that respect and given that this paper is the first attempting to derive partial ice-type concentrations for Antarctic sea ice, it would make a lot of sense to provide an adequate review of the difference in the sea ice AND snow properties year-round between the Antarctic and the Arctic that is back-up very well by a convincing set of references. This paragraph does not fulfil that role and should be re-written. --> GC1 / GC2

See our answers to GC1 and GC2

(3.) L47-49: "The ice cover ... The turbulent ..." --> I encourage you to provide 1-2 references each that underline these statements - particularly the notion that Antarctic sea ice is rougher - but also the evidence that the sea-ice structure is often different in the Antarctic compared to the Arctic.

We have underlined the fact that is is rougher and that is has a different structure by three references.

(4.) L53/54: "Beside MYI ..." --> It would be very important to underline that in fact a substantial amount of the MYI along the East Antarctic coast is actually fast ice. This is often true (older than 2 years old) multi-year ice and is of even larger importance for the ecosystem and has effects on buttressing the ice shelves.

We have inserted this information and added a reference (Massom et al., 2010).

(5.) L55-56: "pancake ice can form" --> Isn't this underestimating the fact that a lot of the Antarctic seasonal sea ice is actually formed via the so-called pancake ice cycle first published (in the 1990ties or late 1980ties?) by Lange et al. ?

We agree with the reviewer. Sea ice is formed in the Antarctic under turbulent atmospheric and weather conditions. Hence, pancake is common. Once again, it is possible to include pancake ice as a separate entity in ECICE using samples from authentic data. But this goes beyond the purpose of the manuscript. We admit, however, that pancake ice can be confused with MYI if radar data is used alone. But the combination with passive microwave can help. We have not done work to confirm this matter.

We now mention the importance of pancake ice and refer to the Lange et al (1989) paper, and raise the topic again in section 3.2 (now L397)

(6.) L60/61: Please check whether it is really the sea ice type that is required or whether these models wouldn't primarily be happy with using improved data of the sea-ice thickness (distribution), the degree of deformation and the snow load. Also, when it comes to validate a climate model I suspect that there are very few that already provide "sea-ice type" as a variable. They might provide ice age though.

We have checked that. We have reworded the sentence, saying that detailed information, among the ice type and thickness, are needed to better validate and improve models. If a model provides, e.g. ice age, this can be set into relation with the ice types FYI and MYI.

We think that mapping ice types that are characterized by different thermodynamic and emissivity is what models really need. ECICE is a generic method, which is capable of producing this information. We have not tried it because we follow the traditional WMO age-based ice type so far.

(7.) L64++: "Recently, ..." --> While it is ok to already mention ECICE here, I ask you to provide a bit more background about algorithms that have been developed in the Arctic to separate FYI from MYI and to provide MYI concentration - first and foremost the NASA Team algorithm.

In addition to that, in order to put the value of your work into a wider context, I also ask the authors to provide more background about other attempts to discriminate between Arctic ice types. It is important that the reader understands that there is almost a full zoo of methods focusing on discriminating between different ice types in the Arctic - in addition to the NASA Team algorithm. To mention in addition to your and Ye's work is the work at met.no, at IFREMER, at BYU (David Long and his group) (and possibly others) that use coarse resolution satellite observations followed by the uncountable attempts to discriminate ice types using SAR. In contrast, activities in the Antarctic are very sparse.

You might argue that you are looking for ice type CONCENTRATION and not a simple discrimination. That is true, but even here your work is more upfront than any other work and this needs to be (implicitly) stressed.

You might also argue that ice type CONCENTRATION is the more important parameter, but if I understood your introduction so far correctly, then we are in need of ANY information about the ice-type distribution of Antarctic sea ice (other than land-fast sea ice), no matter whether this is a binary classification result or whether it is (already) an ice type concentration.

Because of this I ask you to one more time dig into the literature and try to find out what others did in this sector. If we omit polynyas / fast ice - for which a lot of studies exist - then there is not too many, perhaps add: Lythe et al., Classification of sea ice types in the Ross Sea, Antarctica from SAR and AVHRR imagery, International J. Remote Sensing, 20(15), 3073-3085, 1999, http://dx.doi.org/10.1080/014311699211624

and Ozsoy-Cicek et al., Intercomparisons of Antarctic sea ice types from visual ship, RADARSAT-1 SAR, Envisat ASAR, QuikSCAT, and AMSR-E satellite observations in the Bellingshausen Sea , Deep Sea Res. II, 58(9-10), 1092-1111, 2011, https://doi.org/10.1016/j.dsr2.2010.10.031 --> GC2

We have included a brief discussion on other attempts of ice type discrimination (but would refer to the introduction of Ye at al. 2016a and b for details about that in the Arctic). We have included the above references concerning sea ice type discrimination in the Antarctic.

(8.) L84/85: "It takes input ... any given ice types" --> So, I can input 6.9 GHz AMSR2 TB H-pol and 5 GHz ASCAT observations and can obtain the partial concentration of pancake ice? Or I can input 91.6 GHz SSMIS TB at H- and V-Pol and get the partial concentration of MYI ice and FYI ice? If this is not the case then I suggest to re-write this sentence according to the actual capabilities of ECICE which seems to be oversold a bit here.

The sufficient number of channels is the necessary condition (number of input channels greater or equal number of surface types). ECICE is a generic algorithm, so we can in principle use any input channels. However, if the probability distributions of the different surface types in the channels do not differ enough, the retrieved results have very high uncertainty and might be meaningless.

(9.) L87/88: "to account for anomalies ... One anomaly causes ..." --> I suggest to re-phrase these statements. It is not clear what you mean by "observations". To me observations are the data you obtain from the satellites, i.e. brightness temperatures or backscatter coefficients. Hence, I ask myself what anomalies are in this regard? You possibly refer to those cases where ECICE fails to interprete the input satellite data into the correct total and/or partial ice concentrations, creating anomalous high or low concentrations and/or an anomalous misclassification of MYI as FYI and vice versa. Therefore it might be more correct to state that one set of satellite observations can be the result of several different combinations of physical parameters, causing ambiguous retrieval of total and/or partial concentrations when input into ECICE.

We rather meant “observations” in the sense of “anomalous ECICE results” and have rephrased accordingly.

(10.) L100-102: Please provide 2-4 references for publications that could underline your statement for sea-ice concentration and sea-ice type concentration - for both passive and active microwave observations.

We now refer to the overview paper by Ivanova et al. (2015): With the exception of ECICE, all 11 algorithms presented there use tie points, including all ``standard" ones like NASA Team or Bootstrap algorithms.

(11.) L111-113: What happens, during the retrieval, if fractions do not add up to 1 and/or for fractions below 0 or above 1? Are these set to 1 (or 0) before the median of all realizations is computed?

This is avoided by introducing the inequality constraint in the constrained optimisation approach of ECICE, as mentioned in L111-113.

(12.) L116: How is the spread around the median computed? How many valid values are required for a median and its spread to be computed (assuming that not all 1000 realizations provide a valid result)?

This is explained in the original paper of ECICE (Shokr et al. 2008) and in the book "Sea ice: physics and remote sensing" (Shokr and Sinha, 2015, chapter 10). We think we do not have to repeat the information in order to not to distract the reader. However, we have introduced a few lines to explain, at the end of section 2.1.

(13.) Lines 119-124: I am missing the physics and references in this paragraph. What are the physical properties of the ice types that cause the different radiometric and backscattering properties that allow us to discriminate between the three ice types? Which of these are influenced by which snow physical properties that make MYI to look like FYI? How about the ambiguities between YI and FYI?

You use snow metamorphism only in the context of "return of cold temperatures" albeit snow metamorphism encloses a wide variety of changes of the snows' crystal structure and composition under the action of temperature, humidity and wind. This should be re-phrased. In addition "warm spells" only cause "snow wetness" to develop if the temperatures are high enough; still, even with considerable below freezing (-5 degC) temperatures snow metamorphism (rounding of grains, etc.) is present. --> GC1

These five lines are only the very brief introduction to the correction schemes described in detail in the next two subsections (including the references with more details on the correction schemes). Stating here that melt-refreeze causes snow metamorphism does of course not imply that snow metamorphism can only be caused by melt-refreeze- we have added a brief statement to clarify that.

(14.) L126-134: This paragraph describes the temperature correction as developed for Arctic conditions. You appear to adopt it 1-to-1 to Antarctic conditions as is indicated by the last sentence in this paragraph. Without an adequate introduction and review of the physical, radiometric and backscattering properties of Antarctic sea ice and its snow cover compared to the Arctic, this raises my concerns. On the one hand differ Antarctic MYI and partly also FYI physical and microwave properties from Arctic ones. On the other hand differ Antarctic snow properties often fundamentally from those in the Arctic - not to speak of the frequency with which the weather influences the microwave signature of Antarctic sea ice compared to the Arctic. I am wondering whether a close collaboration with specialists in this field would not substantially improve both, set up of the algorithm and interpretation of the results.

Using the same setting as for the Arctic is a preliminary approach. As stated in our response to GC4 (see above) this study is a "proof of concept". As to the underlying physics, see also the response to GC1, as to the choice of parameters, see items (19.4) and (20.).

(15.) L150-152: "this correction scheme ... to MYI" --> I suggest to separate this correction from the drift correction because it has nothing in common with it. I further suggest that you make clear which form of snow metamorphism you are refering to here. In L154 you introduce "HR" as being related to the "onset of snow melt" which, at first glimpse, would suggest an increase in snow wetness and hence elevated brightness temperatures, making MYI to look like FYI rather than the other way round as is stated here. You are possibly refering to melt-refreeze cycles or the like and need to specify this here to avoid confusion.

The additional correction referred to here needs the "MYI domain" of the so-called "drift correction" and is done in the same step, therefore we do not consider it a separate correction. Note that the "drift correction" (in spite of the name) also corrects for the effect of snow/ice metamorphism as it eliminates ice that "looks" to the microwave instruments like MYI but is not, which we have clarified now (end of section 2.2.2). In other words, the drift correction uses the ice drift data to make sure that MYI does not appear very far from its expected domain of expansion (it does not correct for drift or something like that). We have clarified this at the end of the section.

(16.) L150: To me "Ex-MYI" implies that this sea ice once was MYI and now is a different ice type. How about you name it "artificial MYI"?

We have looked for a more appropriate term and now call it "non-MYI", as all we know is that it is not MYI.

(17.) L166-169: I suggest to add the actual resolutions and sampling interval of the AMSR2 channels used.

Please provide information about the native spatial resolution of the ASCAT data and how you gridded these into the NSIDC grid of 12.5 km grid resolution. It appears to me that the statistics is different for these data than for the AMSR2 data because of the different viewing geometry and swath width.

What would also be important to know is whether the sigma_nought values were corrected towards a certain common incidence angle (e.g. 40 degrees)? If this is not the case, please provide a comment why you deemed that as not being necessary.

Yes, we agree this information is missing and have added it here (section 2.3, below equ. (2)).

(18.) L170-180: Your description about the choice of sample areas and time periods is not specific enough to my opinion. I have the following questions:

18.1) Apparently you used ASI SIC maps to define your sample areas. What is the requirement regarding the SIC to have a grid cell contributing to the sample?

Near 100% SIC, of course.

18.2) How did you define "beginning of the cold season"?

For the purpose of finding suitable sample areas, the beginning of the cold season is the time when regionally the sea ice concentration and extent start to grow again, this can be directly determined by visual inspection of the daily ASI maps. We have included this information in a footnote.

18.3) For which time period (just 1 day?) did you select grid cells from the Weddell Sea defining the MYI distribution?

18.4) For which time period(s) and region(s) did you select grid cells defining the FYI distribution?

18.5) From which time period(s) and region(s) did you define the YI distributions based on the PSSM data set?

We have included additional information on that (new Appendix A)

18.6) How did you take into account the YI that develops during the ubiquitous pancake ice cycle in the MIZ that might cover several hundreds of kilometers? Isn't this, not the one growing in the polynyas, the far more relevant YI type in the Antarctic?

Good point. This is discussed in the discussion section. Taking this into account in order to get better distributions is the next thing to be done (after this proof of concept), mentioned in the list of future work at the end of the Summary/Conclusion section.

18.7) Where exactly, with respect to the ice edge, are your open water sample areas located?

18.8) How representative are the open water sample areas in the regions and months (March, Ross Sea; August, everywhere?) chosen for the weather influence?

One way to answer at least some of these questions would be to create a map in which you show the locations of the sample areas and, via color coding, the time-periods and/or frequency with which you used selected the data.

We have specified the details on the sample area selections and locations in Appendix A.

(19.) Table 2:

19.1)- I have concerns with two values in this table. Why do you define the END of the warm episode with a positive (2degC) air temperature? Is this a typo? If not it is absolutely not understandable and needs some justification.

There is actually a typo: the T1 must be -2°C and T2 +1°C. Note that the condition for the start of a warm spell is T>T1 *and* a reduction of MYI concentration in one day by 10%, and the condition for the end is T<1°C *and* a rise of MYI concentration by more then 10% in one day. Only in such a case, the MYI concentration is corrected. T1 and T2 have been found empirically, as described in detail by Ye (2016a). Note also that T1 and T2 are 2-metre air temperatures, not ice surface temperatures.

19.2)- What is the motivation for the very long maximum duration of the warm episode? This does not sound overly reasonable to me - neither for the Arctic nor for the Antarctic actually. I can guess that the length of this period is chosen this way because the melt and melt-refreeze processes change the physical and therefore microwave signature of the snow / sea ice system for a considerable number of days; even after freezing conditions have returned the modified microwave signature might still last (e.g. Voss et al., 2003, in Polar Research and his work related to that).

Even though most warm episodes will last only few days, there is no harm in setting the *maximum* duration to 10 days in order to also catch rare longer events. It is computationally more expensive, but not in a significant way.

19.3)- Apart from that I am wondering how such a long maximum duration does match the comparably high frequency of warm events caused by cyclones passing over the sea ice. I'd say that such events can be quite short-lived. Therefore, depending on whether you aim for a monthly or a daily ice type product one could recommend to use a considerably shorter maximum duration of such events of just 5 or even 3 days.

Yes, a reduction of the maximum duration will reduce the latency times if we want to do NRT data, but that was not a concern of the present study.

19.4)- In case you comment on the choice of these parameters later in the paper, i.e. in the context of the discussion, please point the reader that already here to increase the credibility of your choices.

See item (20.)

(20.) Table 3:

- I note that the choice of the values for these parameters specifically for Antarctic conditions has not been discussed and/or movitated so far. You might want to do that, please.

- In case you comment on the choice of these parameters later in the paper, i.e. in the context of the discussion, please point the reader that already here to increase the credibility of your choices.

We have mentioned that for the time being the same “tuning” parameters have been used as in the Arctic and fine-tuning might improve the results (section 2.3, L251ff.). However, other issues (pancake ice, outer Ross Sea) appear more important to be resolved first

(21.) L187-191: Three comments here:

21.1) What kept you from using ERA5 data? Is there are credible argument to stick to ERA-Interim data for surface temperature data in the Antarctic?

When most of the presented work was done, ERA-5 was not available yet.

21.2) Tschudi et al. (2016) appears to be a bit outdated given the fact that there is a version 4.1 of the NSIDC sea-ice motion data set, referenced as Tschudi et al. (2019 or even 2020).

We have updated the reference.

21.3) What is the motivation to use this rather low resolution OSI SAF sea-ice drift product? Doesn't it harmonize with the overall 12.5 km grid resolution you aim for much less than the NSIDC sea-ice drift product?

The source of the drift data was decided at the start of the study, several years ago. At that time, it seemed a good solution (probably,the temporal/seasonal coverage for Antarctic was better at that time). We are actually considering switching to NSIDC drift data for the future (see end of Summary/Conclusion).

(22.) L192: In Table 3 you mention the TB at 37 GHz, not 19 GHz; please check.

This is a typo. Thanks for finding it, we have corrected it.

(23.) Figure 1: I have a number of comments here; comment #1 and #2 are related directly to the figure content while comments #3 to #5 are related to the omission of relating the results shown to previous work.

23.1) What does the "Distr. set: AQ2" in the title of each panel refer to? Could it be removed?

(Note: now Figure 2) Yes, this was a working title which has been removed.

23.2) What is the statistics behind the data? What is the time period? At how many data per surface type do we look?

We now list these details in Appendix A.

23.3) What explains, to your opinion the fact, that GR3719 is a bit lower than is classically observed for open water and, particularly, for FYI (compare the tie point triangle used in the NASA-Team algorithm).

This probably depends on the choice of open water samples. We will elaborate on the sample choices anyway (see item 18.)

23.4) How do your values compare in general to tie points used by ordinary sea-ice concentration retrieval algorithms?

23.5) How do your backscatter values compare to values for C-Band radar backscatter of Antarctic sea ice cited in the literature?

We have included a brief discussion of these two points (L230ff.).

(24.)L201-203: While I am fine with using AMSR-E instead of AMSR2, I have concerns to simply replace ASCAT (C-Band) with QuikSCAT (Ku-Band) as signal penetration into and interaction with the snow / sea ice system differ - in addition to incidence angle and resolution.

Yes, correct, one cannot simply replace the scatterometer data. The respective distribution functions for the surface types are needed as well. Retrieval with QuickSCAT instead of ASCAT and with AMSR-E instead of AMSR2 has already been done (see Ye, 2016a) – we have pointed this out in the manuscript (end of section 2.3)

(25.) Instead of working with piece-wise available sea-ice drift products it might be a very good idea to use one consistent data set, namely the NSIDC one - unless you find an alternative with year-round coverage (IFREMER?); yes, NSIDC is not an optimal choice but with that you avoid inconsistencies and jumps in your then much longer (by combining AMSR-E and AMSR2) time series. You might want to consider to simply delete these three lines here.

Yes, of course. If there is a consistent time series of drift data for the whole period, all data should be processed and reprocessed using that one. We keep theses three lines without extending them because we would just like to emphasize the potential/perspective of a longer time series.

(26.) Section 3.1:

(26.1)- How many Sentinel-1 SAR data from which dates were used? Where were these located (provide a map with the frames)? What was the time difference between SAR image acquisition and ECICE product? What is the "time stamp" of the ECICE products [0 UTC, 12 UTC]?

We use daily gridded brightness temperatures (see L166ff. of original manuscr.), interpolated from all swaths of one day (details on that will be included) – so there is no unique time stamp. This is now explained in the text.

(26.2)- Where were the Sentinel-1 SAR images taken from. Which type of SAR images was used (Wide Swath, Extended Wide Swath, ...)? How were the SAR image (pre-)processed for the evaluation? Was any drift correction applied to the SAR images?

We have included the missing information.

(26.3)- You decided to provide a qualitative intercomparison without computing radar backscatter (sigma_nought) values. Why? Wouldn't your results be much more credible and useful if you would come up with fractions of MYI and/or FYI derived based on a rough (by means of sigma_nought value) classification from the SAR images and compare those to the ECICE MYI concentration maps?

See response to item 26.7

(26.4)- L214-215: "In SAR images, MYI ... sub-surface layer" --> This very qualitative and not overly scientifically formulated sentence applies to the Arctic. Melt processes during summer in the Antarctic differ considerably from the Arctic and I doubt that one can speak of a "bubbly sub-surface" layer here. Please revise your wording taking int account the specifics of seasonal changes in microwave signatures in the Antarctic compared to the Arctic.

We have revised the sentence in question (now L284-288).

(26.5)- L221/222: "the iceberg ... as FYI" --> I don't agree. The SAR signatures inside that 70% FYI polygon encircling the iceberg are brighter than outside the polygon. The isoline does also not indicate at which side FYI concentrations are actually higher or lower. Given the fact that the area southwest of the iceberg is certainly dominated by FYI I suggest to re-phrase this statement along the lines that for that polygon both FYI and MYI concentrations are below 70% but that you don't know which is the dominant one. See also your Figure 5.

Yes, we agree and have corrected that error as suggested. Actually, looking at the individual maps of FYI and MYI, the icebergs tend to have 0% FYI concentration, which we now point out when discussing the comparison with SoD charts (now Figure 4).

(26.6)- For one grid cell, do partial concentrations sum up to 100%? I am asking because in the area indicated as > 50% YI fringing the Antarctic Peninsula there is evidence for MYI concentration > 50%. Did you actually check for maps like the one shown in Fig. 2 what the sum YI + FYI + MYI concentration is? It would interesting to see an example of this - perhaps in the appendix or in supplementary material.

See response to GC4 (4th paragraph). The sum is the total ice concentration (between 0% and 100%).

(26.7)- L222: This last sentence about the "quality" of this comparison I deem almost obsolete without information about how many SAR images of how many regions from which dates have actially been taken into account.

We will include more details about the number of scenes compared. We admit that the comparison is not very detailed, but rather an initial sanity check that focuses on the rather prominent MYI-FYI boundary usually found in the inner Weddell Sea – we will reword the sentence in this sense.

(27.) Section 3.2:

(27.1)- How are the weekly charts derived with respect to temporal availability of the input data? Is always the latest highest quality data set used for a respective grid cell (or pixel)? Or what is the compositing method used?

(27.2)- Does "microwave satellite imagers" include SAR? What is the dominant input data source for the charts you have used?

(27.3)- What does "analysis ... by experienced specialists" mean? Is this a manual analysis? Is the analysis done by one specialist or a team of specialists and what are the quality measures?

(27.4)- L228/229: What is the size of such a pixel? What is the grid that is used here? Is it the NSIDC polarstereographic one? Looking at Figures 3 and 4 I get the impression that in these ice charts the classification is not done pixel-by-pixel but rather in form of polygons that contain ice of similar characteristics and concentrations - such as done, e.g., by the Canadian and Danish Ice Services. Could you please check once again how the ice charts you show in your manuscript were generated, and if need be, re-phrase your description?

(27.5)- Were the input data projected into a common grid prior to ice chart generation?

(on 27.1 – 5) Ice charts are generated based on SAR image analysis as the prime data sources but combined with many sources of ancillary information. SAR images are analysed visually but ice analysis operators. One operator analyses each image, hence the analysis is subjective. However, those analysts are well trained and experienced. The ancillary data include climatic information about the area, the recent history of the ice filed, meteorological data, observations from ships and ice breakers Such charts consist of polygons, not pixels, thus we have reworded the sentence mentioning "pixels" which solves item (27.4)

(27.6)- L234/235: What is your "cold season"? What do you mean by "sporadic comparisons with data from other years"? How many, from where and which dates were these additional comparisons?

By "cold season" here we mean the period when the algorithm works because there is no widespread and sustained surface melt, i.e. March to October (inserted in the text now). We still consider adding all comparisons as supplementary data.

(27.7)- L243-246: Your observations of the different labeling of ice as FYI or MYI between AARI and NIC charts could be the result of different definitions of when FYI is re-labelled MYI ice by the producing agencies? Did you check that? I note that a switch in March / April disagrees with the WMO recommendation you mentioned further up in your manuscript.

We think this rather shows that the two different teams of ice analysts came up with different assessments of their data, maybe also because they use different data. This is hard to find out.

(27.8)- I note in addition that there are more fundamental differences between the AARI and the NIC ice charts in the Eastern Weddell Sea regarding the location of YI and FYI.

See above...

(28.) Figure 3:

- I suggest to use a title for the MYI concentration that is consistent with the other two ECICE results.

(Note: now Figure 4) We have removed the small titles of the sublplot and instead put the labels YI, FYI and MYIc into the centre of the subplots.

(28.2) - Putting the legend of the AARI ice chart into appendix is not a good solution. I suggest the following: You crop all maps to an area that excludes all the annotations in the AARI ice chart, put all ECICE results into the second row of panels and put the ice chart legend in the first row of panels next to the ice chart. In the second row of panels you could then also follow your approach from Fig. 5 and provide one legend with the title "Ice-type concentration", marking the ice type itself in the map (actually it is in the panels' titles but perhaps you consider to remove these anyways.).

We have considered the suggestion. However, note that the SoD charts use one “color family” for each of the three broad ice types: pink/purple hues for YI, yellow/green hues for FYI, brown hues for MYI, and this is the information needed here (given in the caption). We think the full color scale with all the subtypes would be too distracting here.

(28.3) - Finally, I note that you seem to use an old land mask to mask out Antarctica, still containing an overly long "Trolltunga" of the Fimbul Ice Shelf and the Mertz Ice Shelf. Given the fact that you focus here on AMSR2 data it might be a very good idea to use an more recent and hence more accurate land mask.

We have used the land mask that comes with the AMSR2 data (!), but see the need to improve that in the future.

(28.4) - L258 / Fig. 5: "with the summation of their fractions equal to 100%" --> When I look at Figure 5 I doubt that this statement holds. I guess it needs to be replaced by the actual total sea ice concentration that is obtained with the ECICE algorithm because there are quite some areas downstream of the Ronne-Filcher Ice Shelf polynya where YI conc. + FYI conc. + MYI conc. add up to something between 80 and 90%. I am sure you will get back to this in the discussion section. But it certainly does not hurt to either state that "theoretically" the partial concentration should add up to 100% but that this is not always the case, or correct your writing accordingly towards that the sum of the partial ice concentrations adds (of course) only up to the actually existing amount of sea ice. --> GC4

(Note: now Figure 6) As stated before, the sum is 100% only if the open water fraction is added as well. We have inserted “(including the open-water fraction)” to avoid misunderstandings.

(28.5) -L261/262 / Figure 5: Your maps do also reveal that ECICE seems to have a problem discriminating between YI and MYI because the area just next to the Ronne-Filcher Ice Shelf appears to be characterised by some YI, no FYI and some MYI as one can observe a fringe of non-zero MYI concentration in that area.

(Note: now Figure 6) This is another hint at revising/improving the input parameter distribution, in particular of YI - this is now mentioned in the discussion (now section 3.3, L394ff.).

(29.) Section 3.3:

(29.1) - Please provide more information about the PSSM maps. What are the grey areas masking parts of the maps shown? Where did you get the data from? What is their temporal and spatial resolution? How many of these maps did you use for which regions? The scope of this part of your intercomparison remains vague.

The grey areas are masked-out areas (land, of course, and apparently the sea further away from coasts, depending on the definition of the regions covered) - we now mention this in the text and the figure captions. The source of the data was already given above, section 2.3, (originally L175-180) and has been extended for the revision. We will put in a back-reference here.

(29.2) - Given the fact that you look at years 2017 and 2018 I assume it is SSMIS data and not SSM/I anymore, am I correct? Please correct your writing accordingly.

Yes, we have corrected that.

(29.3) - I would appreciate if you could comment on the quality and limitations of the PSSM based ice type maps. What the approximate thickness limit between thin ice and "other ice"? Does "thin ice" mean that there is 100% thin ice or could this potentially also be 50% thicker ice interspersed with open water?

We have inserted this information. The thickness limit is, according to the data provider (ICDC, CEN, Univ. of Hamburg), 10–20 cm.

(30.) Figures 6 through 9:

(30.1) - I suggest to reduce the size of the panels considerably. In particular I recommend to make the PSSM map the same size as the white box shown in the left panel denoting its location. Even better would be if you'd crop the maps in the left panels to the size of the PSSM map. That way would would be able to reduce the number of these figures from 4 to 2 or perhaps even 1. Did you try, in this context, to combine the information from both panels into one? Perhaps by extracting isolines from the PSSM maps and superpose these onto the YI concentration maps?

(Note: now Figures 7 to 10) We have considered the suggestions. However, If we crop the YI map on the left, to just the white box, it is not easy to see which part of the Antarctic is shown, therefore, we would rather keep the large map. If, instead, we reduce the size or the PSSM map on the right, we would not really save space as the smaller maps also need the right half of the figures, so why reduce the size.

(30.2) - Did you chose the dates shown in the manuscript arbitrarily? If so, make a note.

(30.3) - Were these the only PSSM maps you considered in your comparison? If not how did the comparison go for all the other maps? Did you derive any quantitative information?

We just show a few representative examples.

(30.4) - Looking at these YI concentration maps reminds me one more time the issue of how the ECICE ice-type concentration maps deal with cases of considerably less than 100% total sea-ice concentration because I note that all the YI concentration maps shown in Fig. 6-9 reveal lower YI concentrations in the core of the polynyas.

The partial ice concentrations of the three ice types sum up to the total ice concentration which is between 0% and 100% (this is now explicitly mentioned in section 2.1). We do not see a problem here.

(31.) L280: "given the limitation that a rigorous validation ..." --> Given the fact that you kind of advertise the data set obtained with this paper and given the fact that this is first attempt to provide such a data set, I don't take it as a positive sign of credibility of the data set produced, when this paper only deals with a very general, little quantitative evaluation. The results presented are partly very vague and the description of the physical background being the foundation for the approach used and the data set is not overly exhaustive and - at least for me - not convincing.

See response to GC4 (aim: proof of concept)

(32.) L281: "Large icebergs are often erroneously retrieved as FYI" --> Is this your result? You could state this more clearly. But, when doing so, please take into account my comment made to Fig. 2 with respect to this issue.

As already mentioned above (see item 26.5), this was an error on our side. We have corrected that statement (now L358ff.), and emphasised that icebergs and their discriminations are not our focus here.

L288-300: I was kind of expecting that you would run into problems with weather-induced variations in the snow physical properties and resulting microwave signatures. Since in your manuscript the physical foundation and description of the processes and properties resultung in specific microwave signatures is not overly detailed and mature, it is of course difficult to discuss these observations. I find that your attempt to explain your observations go into the correct direction but are far from being conclusive and is too vague. I'd say you could delineate the reasons that caused the MYI concentration over-estimation much better and much more specifically by means of checking the input data values and compare these with what is known from literature. It might make sense to take into account ERA-Interim and/or ERA5 data (you use them anyways) to discuss you observations also in the context of melt-refreeze, ice-snow interface flooding, slush refreezing, snow-ice formation and the like. I again recommend to take a look at the work of Voss et al. (2003) and the related doctoral thesis.

As to the physical foundations, see our response to GC1. We admit that we can improve on the interpretation of the results and thank the reviewer for the suggestions. However, any erroneous increase of MYI during the cold season because of changes in the snow and ice properties should be removed or at least greatly reduced by the drift correction — therefore our initial concern is why that does not work which has nothing to do with the physical properties of the sea ice and snow. As already mentioned later in the manuscript (L310ff. in original version), instead of working on the correction scheme, it might make more sense to prevent the misclassification by ECICE, which means to use better samples representing YI, FYI (and MYI) — here knowledge of the range of radiometric and scattering properties of FYI might indeed be useful.

(33.) Figure 11: Looking at that figure again makes me to think whether you ever tried to look at maps of YI conc + FYI conc + MYI conc? It appears to me that there are patches of spuriously large MYI concentration that coincide with a total sum of partial concentrations above 100%.

(Note: now Figure 12) Well, as we only modify the MYI concentration in the correction schemes. which breaks the sum rule that the partial concentration *plus the open water fraction* add up to 100%. The problem is that we have more than 2 ice types, so if we increase/reduce MYI concentration, it is not clear how to distribute the corresponding reduction/increase to FYI and YI, so we refrain from it. If we just had one ice type apart from MYI, it would be easy to preserve the sum by just mirroring changes of the MYI concentration.We have, by the way, checked that the sum of the uncorrected ice types is indeed the total ice concentration (see also the new section 3.3 on the time series).

(34.) L299: None of the references listed in this line deal with pancake ice and its backscatter. These are all references dealing with the snow cover and should be put into L298 behind"... MYI in that respect."

Yes, corrected.

(35.) L292: How credible are - to your opinion - these MYI occurrences "far offshore in the outer Ross Sea"? Which process can cause these?

The “streaks “ of MYI be found in some years in this area (according to theNSIDC/AARI charts) seem to have drifted there from the East, so they originate as MYI near the coast of Wilkes Land and get to the outer Ross Sea by advection, which seems quite possible. However, their total area must be equal or less than the area of MYI of the source region.

Final question to L288-300: How did you compute the total MYI area shown in Fig. 10? Did you apply a threshold MYI concentration or did you count from 1 % onwards? What did you use as gridcell area to compute the total area?

As stated, we calculated the area, which means we take into account the MYI concentration from 0% to 100% and the (space-dependent!) grid cell sizes of the polar stereographic (NSIDC) grid (clarified, now L365ff.).

(36.) L302-303: You can look yourself into the likelihood of (1) by checking the drift data you used. How did you cope with data gaps in the drift product? Did you include the quality flags?

Well, data gaps (e.g. NaN values because of insufficient cross-correlation) will cause the MYI domain to not be extended, causing a rather strict correction. As mentioned below (item 38.), the drift data are the next thing to be updated.

(37.) L306-307: "such seeding points" --> please explain this in more detail or delete it. Questions I would have is how this happens and why this should have an influence on the MYI concentration in particular and not on the other partial concentrations.

We have improved he explanation (L384ff.).

(38.) L309/310: I don't understand why you refer to an observation of Ted Makysm when you yourself used the data for the drift correction. Didn't you yourself take a look at the data once you suspected that these could include spurious drift estimates? This is inconclusive.

As now stated in the discussion, before doing extensive error search in the drift correction, the thing to do first is improve the distributions of YI and MYI in order to mitigate blatant misclassifications.

The next step would be then to use a newer version of drift data (OSI SAF or NSIDC); this is mentioned in the Summary/Conclusion.

(39.) Line 311-314: I suggest to not look into the data used but first try to understand which sea ice and snow physical properties you encounter during the course of one cold season and to further understand how the microwave signature looks like. This might require to look into 1-dimensional numerical modelling of microwave emissivities and of microwave backscatter as a function of sea ice and snow properties. There is a paper by Willmes et al. (2014) in the Cryosphere and there is work by Tonboe et al. that might help here.

We will look into that matter, but a dedicated modelling study is clearly beyond the scope of this manuscript (but is worth doing in the near future).

(40.) L319-322: "The most likely reason ..." --> While your observation from Fig. 10 seems to be credible, I am wondering whether this isn't an over-simplification of the situation. I agree, wettening of the snow cover can mask MYI so that it looks like FYI. But at the same time the re-freezing of the slush at the ice-snow interface, ice lenses, whatsoever causing larger grain sizes can have the adverse effect and making FYI looking like MYI. A deep snow pack and/or substantial deformation of FYI has the same effect as demonstrated by one of the co-authors for the Arctic ocean. In addition, and here the authors were right earlier of course, pancake ice is a nasty fellow and could possibly also likely to be misclassified as either of the two thicker ice types - adding to their partial concentration.

We agree with the reviewer's points. The physical processes of the snow and ice modulates the radiometric and the scattering data. However, the advantages of using the probability distributions of all possible values of a given observation from a given ice type warrants the inclusions of all possible conditions. Sure, the snow wetness and refreezing changes the observations but if the input distributions encompass all the possible changes, then the correct classification is warranted. As for the point of possible misclassification of pancake ice as MYI, we have not considered it. The two entities may not be misclassified using the present data set because while they have nearly same backscatter, their radiometric emission is different. This is an advantage of using the combination of passive and active microwave. Pancake ice is not part of the purpose of the study but its confusion with MYI should be considered. Here, ancillary information is required to avoid the inclusion of pancake ice areas. See also response to (18.6) above.

(41.) L323-326: Two more thoughts on this: Beginning in October the expansion of the Antarctic ice cover stops and the lateral movements switches to a retreat / compaction type. In addition, due to the dispersion the fraction of MYI per grid cell has decreased to a value that is likely not large enough anymore to be adequately detected by ECICE. I am sure this is something you can check in your data. One could hypothesize that computing the total ECICE MYI area is reasonable as long as ECICE is capable to derive the MYI concentration with high accuracy ... which I doubt is the case when the partial concentration has fallen below 30% and when the MYI coverage has dispersed in many small floes embedded in a mixture of YI and FYI.

Also any MYI that has arrived in the MIZ (in the Weddell Sea) is now likely to melt as air temperatures are not cold enough anymore out there to keep it alive. From that point of view I find a rather decay of the MYI area in the September / October time frame not overly surprizing.

The accuracy of the results from ECICE has not been estimated quantitatively because this requires in situ observations. This statement applies to other algorithms too (e.g., MYI concentration from NASA Team algorithm). We do not know the minimum concentration that can be estimated but the manuscript provides data about the entire range of concentrations. Operational ice charts cannot be used for this purpose (in our opinion) because of their coarse resolution, subjective method and most importantly the conservative estimates in these charts. Given all this, we don't think that the partial concentration is inaccurate if it falls below 30%. The limit should be lower than that.

(42.) L337: "and melt" --> Where did I find examples of these in your manuscript?

Maybe the figure in the manuscript do not show this very well. When looking at the daily evolution of MYI over an entire season, this is quite obvious. In view of this comment, we still consider including an animation of one season in the supplementary material.

(43.) L343-344: "The new time series ... outweighs the shortcomings that still exist." --> I do not agree to this statement because of 1) the unmature physical foundation, 2) the vague interpretation of spurious ice type concentrations and 3) the very qualitative evaluation.

We still think that this data set, which we will term "preliminary", has its merits (it has actually already been used by Antarctic Cruises of the University of Cape Town) and serves as a proof of concept (see also response to GC4). We have rephrased accordingly.

Typos / Editoral Comments:

L27: I suggest to look for a more recent paper making this statement, e.g. Kwok 2018 in Environmental Research Letters.
L30: There should be another reference from Parkinson and DiGirolamo from 2022 in Remote Sensing of Environment.
L49: Typo: "...sea ice For ..." --> "... sea ice. For ..."
L79: "existing a ice chart" --> "existing ice chart"
L114: Typo: "coast" --> "cost"
L152/153: Please explain all the mathematical expressions that are used here for the first time.
L159-164: You have introduced the sensors' acronyms further up and can omit that here.
L207: I am sure a reader would appreciate to see 1-2 references here. - Validation is just starting, no references yet..
L210/211: It might make sense to mention already here that the polynya maps used in the comparison are from a different year than those used for algorithm tuning.
L222: "Sentinel-1 scenes" --> "Sentinel-1 SAR scenes"
L256: Typo: "forth" --> "fourth"; see also L262
L266: Typo: "where" --> "were"
L286: "often" --> since you deal with a limited number of years in this paper you could perhaps mention all years during which you observe this increase.
L332: "outside the melt season" --> "during the freezing season"
"spatial" --> "grid"
L334/335: "... is well captured" --> You could add "by our ECICE results" to make clear that this is your result.
L334: "... in the Antarctic" --> add: "in addition to ship-based observations of the ice conditions."

We have corrected the listed typos and errors

Response to RC2:

Note that we quote the reviewer's comments and suggestions in red.

Major comments:

Writing style

The manuscript would benefit from (and strongly needs) a thorough tightening up of the writing style of the whole manuscript, including but not limited to:

● Use a clear structure with separated sections for the data and the methodology. At present, the input data and input products are presented “here and there” within the method section.

We have tried to make a clearer separation between data and methodology. Having two separate sections "Data" and "Methods", however, does not seem very useful to us, as mentioning all data *before* explaining the method is much information that cannot be used yet, and mentioning the data *after* describing the method would be too late. We have included a flow chart (at the end of section 2) that shows the methods (ECICE and the correction schemes) and the various input data and their flow.

● Avoid repetitions. Several repetitions occur and sometimes a simple re-ordering of the sentences would make the reading flow better

● Make sure that information given across the manuscript is in synergy with itself. For instance, it is very unclear whether the new product covers 2013-2019, 2013-2020, or 2013-present.

It is actually 2013 to 2021. Corrected in text.

● Make use of general spell checking.

We have tried to straighten the text (at the same time, however, incorporating a large number of suggestions by another reviewer), eliminated inconsistencies like the one mentioned, and, of course, have applied a spell checker again.

The presentation of this work should be clearly separated from possible improvements or potential future works. Future works or possible upgrades should rather be listed and discussed in a discussion section or the Conclusion.

We think that mentioning some possible improvements/potential future works in the course of the paper is unavoidable: For example. when discussing the wrong multiyear ice in the Ross Sea, we of course mention possible ways to mitigate that (L311 ff.). However, we agree that it makes sense to list all possible improvements and future work in one place and now do so at the end of the final Summary/Conclusion section.

Algorithm presentation vs presentation of long-term time series

The manuscript seems to have two main goals. On the one hand, it presents a methodology for mapping the Antarctic sea ice type from remote sensing data. And on the other hand, it presents, for the first time, a longer time series of Antarctic sea ice types. Both these topics are very relevant, however, at the present stage, the manuscript covers these in an inadequate manner.
In order to be a manuscript presenting a new method, the methodology is only superficially described. Below are some specific points to be considered:

We did not want to repeat the description of the ECICE algorithm and the two correction schemes here, as they are described in the respective publications, but we will try to follow the suggestions below.

● I suggest naming the full algorithm which is implemented at the University of Bremen. When reading the manuscript, it is unclear if “the new method” is ECICE, modified ECICE, or ECICE + post-processing. Examples of this:

○ L64: “Recently, a method has been developed ...”. Which method is this? Add a reference.

(The reference is already at the end of that sentence.)

○ L66-69: “The method is based on ECICE ... and a later modification ...”
Unclear if the ECICE has been modified, or if post-processing includes modifications of the outcome...

○ L72-74: “In this study, we have adapted this method to the Antarctic conditions ...”. Again unclear what “this method” is.

○ L85-86: “Our estimation of MYI concentration actually is a two-step procedure that first uses ECICE and then applies two correction schemes ...” Assuming that “our estimation” is coming from “the new method”, here for the first time it seems clear that the method is in fact the ECICE retrieval pluss some post-processing (correction schemes).

○ L193: “The final result of the two-step retrival scheme ...”.

○ etc.

Thank you for pointing this out. We agree that the naming must be consistent throughout the manuscript and have tried our best to do that.

● L114-116: it is mentioned that “the median is used as a measure of confidence of the result for each surface type”. However, this confidence field is never presented and it is not clear if this information is provided together with the ice type product.

This information is saved along with the results of ECICE (we have included this information)

● Section 2.1 is describing the core/backbone of the classification algorithm. I would have liked to see a few equations or illustrations (e.g. flow diagram) of the ECICE methodology. Especially, the paragraph relevant for the Antarctic adaption (L105-117) is hard to read as it is and would benefit from supplementing equations/illustrations.

We are now including a flow chart of ECICE and the correction schemes that in particular shows the various input data (new Figure 1). The ECICE algorithm is described in detail in the cited publications (Shokr et al., 2008 ; Shokr and Agnew, 2013) and we do not want to repeat too much of that here.

● L166: “For all input parameters, we use daily gridded data”. How did you arrive at these gridded data? Especially, for scatteromter data, a sentence on how the angle-dependent swath data are gridded would be relevant.

We combine all AMSR swaths of one day and then interpolate to the grid using a distance-weighted near-neighbour approach (the one from Generic Mapping Tools with four sectors). The ASCAT swath data are converted to common incidence angle of 40° and then interpolated to the grid. We have added these details (section 2.3, below equ.(2)).

● L175: “Later in the season, sea ice that has formed ... away from MYI is FYI”. How do you account for the changing position of MYI during the season and thereby collect only FYI data?

We made sure the used FYI areas are so far away from the start-of-the-season MYI that the latter cannot have drifted there. If we assume, e.g., 15 km of maximum daily drift (rough estimate from 1 year of OSISAF drift data), this would mean, e.g., at most 1500 km in June. We have added a short explanation.

● L196-197: “... retrieved the ice type concentrations for the months of Feb to Nov ...”.
I did not find any comment on why summer months are omitted, or why exactly this period has been chosen for the Antarctic product.

In general, under permanent melting conditions, the radiometric/backscattering properties of sea ice change considerably and differences between the ice type diminish or even vanish. Therefore using ECICE in summer does not yield reasonable ice types. We apologise for not having stated this important fact and have added this explanation to section 2.1 on ECICE.

● The final output is “corrected MYI” (L193), however the “uncorrected FYI, MYI, and YI” are also provided.

○ It is not clear if the uncorrected fields are “pure ECICE” outcome?

Yes, the uncorrected fields are the “pure ECICE” outcome. We added this were we mention the preliminary ice types (now L254ff., “without applying and correction”)

○ uncorrected MYI (or Ex-MYI) is never presented, and maybe they should be shown?

We have actually already considered that and decided against it as Ex-MYI should first be investigated in more detail. We have renamed it “non-MYI” and now explain a bit more about it where is it first introduced (section 2.2.2, L177ff.).

Please note that other ice concentration algorithms that produce MYI do not apply corrections to account for anomalies in the locations of the MYI. The correction scheme used in ECICE can be used in any algorithm.

○ Could you include a comment whether MYIcorrected+FYI+YI or MYI+FYI+YI add opp to 100%? And if not, please comment on this as well.

The uncorrected ice type concentrations add up to the total ice concentration (which can be between 0% and 100%, of course), When correcting MYI, the amount added or subtracted is not subtracted or added from FYI or YI as we cannot say to which of the latter two it “belongs”. Hence, MYIcorrected+FYI+YI cannot add up to the total ice concentration. We have added this explanation to the text.

● It is not clear if a threshold is used for the ice edge? Or are all surfaces with >0% ice concentration classified?

We do not use any threshold, but directly retrieve the concentration of the three ice types everywhere. In areas of open water (100%), all ice types have 0% of course.

● Several places it is announced that this computation can be extended to present time. What will the latency be for such a retrieval?

We have since implemented daily retrieval. The ECICE output is in near real time, within 1 day of receiving AMSR2 and ASCAT data. The latency of the corrected MYI is 16 days. We now mention this at the end of section 2.3.

In order to be a manuscript presentating a longer time series, it is surprising to see that there is a complete lack of presenting or showing any long-term (seasonal, interannual, regional) behaviour or variability. Only a few hand-picked days are shown and the year 2018 MYI total area time series. Since this is the first time a longer time-series of ice type is presented for the Antarctic, then it would be appropriate to show a full record plot and potentially discuss any trends and variabilities, (or missing trends).

We have added a section (3.3 Time Series 2013-2021) showing complete (2013-2021) time series and discuss them.

Several places it is mentioned the possibility of a record covering the period 2002 to present. However, the present record covers the period 2013-2019. Can you make this more clear - what defines the period you present and why is this period selected? And hereafter (best fitted in a discussion or conclusion section) mention possibilities for extending the time series and what this would require and what is the timeline for implementing this.

We have now stated more clearly (end of section 2.3): The period starts with the availability of AMSR2 data, in principle in July 2012. As the drift correction scheme needs to starts at the beginning of the cold season, we start to retrieve in 2013. The end of the period is now 2021 – we will change the text accordingly.

Other comments

General comment for the figures: Could you consider to label the sub-figures and thereby avoid “ top-right”, etc

The use of “top right” etc. seems common practice in many publications. However, we have now labeled the four panels with (a) to (d) which makes referencing easier.

Figure 1: Why not simply add the sensor type in the title of each subfigure instead of the “Dist. set: Aq2” which is a title that does not give any sense.

(Note: now Figure 2) We have removed the working title “Dist. set: Aq2”. The parameter shown is clearly named in the *x*-axis labels.

Figure 1: Some of the shown density distributions clearly shows a double-peak. Can you comment on this and could this in fact indicate that more types are represented by the distribution?

(Note: now Figure 2) Yes, e.g. the distribution of MYI in GR17,37V, the red curve in the lower left plot, shows two almost distinct peaks. This might point at two subtypes. However, in order to retrieve one more ice type (by splitting “MYI” into two subtypes), ECICE would also need one more input channels as the number of input channels must be equal or larger than the number of retrieved surface types. We are not sure if is makes sense to add this discussion into the manuscript, though.

Figure 2: I find it difficult to locate this region. Either you could add an overview map or with words explain better where this is. E.g. " the inner part of ..."

(Note: now Figure 3)The region is explained in the caption (“Southeastern Weddell Sea, bordering the Antartic Peninsula).

Figure 3: the sub-titles should be upgraded, preferably with one sub-title for each subplot. Also the sub-title of the lower-right should be updated/corrected.

(Note: now Figure 4) Usually there is just one caption under a figure. But we have put labels (YI, FYI, MYIc) into the centre of the subplots.

Figure 6-9: Here is used four figures for illustrating Young Ice. Potentially, these could be merge into fewer figures. The PSSM maps - the gray color should be defined in the caption. Even better, if the coast/land could be added for better orientation of the sub section.

(Note: now Figure 7-10) We have considered combining the figures, but think that this does not make them better to read. The meaning of the grey areas is now explained in the captions and in the text.

Figure 10/L286: What is shown in this figure? Is it the extent of all ice pixels that contain some concentration of MYI? Or is the MYI concentration or the full ice concentration taken into account?

(Note: now Figure 11) As mentioned in the caption, this is the full MYI area, taking the MYI ice concentration of each grid cell (0% to 100%) and the (variable) grid cell area into account. We now explicitly explain that in the text. So this is *not* the extent, which is commonly defined as the sum of the full area of all pixels with an ice concentration above 15%.

General comment for the tables: The layout of the table caption differs between the tables - some times it appears above and sometimes below.

Has been fixed.

General comments:

L25: you could add a reference to the YI definition (e.g. WMO Sea-Ice Nomenclature 2017)

Yes, we add reference to the WMO Sea Ice Nomenclature when we first introduce the ice types.

L33: explain or add a reference to why all MYI is SYI.

We will add the remark that almost all Antarctic sea ice will drift out into lower latitudes and meld within two years (now L36ff.).

L38-39: “Antarctic sea ice has strong region-dependent ...” This sentence stands a bit alone. Could you say a few more words on this?

As this statement is not really relevant here, we rather delete it.

L43: Move “in the Antarctic” to the beginning: “Sea ice cover in the Antarctic...”

Yes.

L51: repeats that MYI typically is in Weddell Sea (L31)

Yes, but we see no problem in repeating this statement here as we here discuss the importance of the Weddell Sea in particular.

L62: “Total and partial sea ice concentration ...” when first time reading this, it was not clear that “partial sea ice concentration” was referring to the ice types. I suggest re-wording this whole paragraph.

We have reworded this: “The concentration of total sea ice and of the sea ice types”

L72: “Ye et al., 2019” is not accepted for publication. Can you find another reference?

A revision of the cited study is in progress, for the time being, there is only the discussion paper.

L74: replace “regularly” with “operationally”

Yes

L77: “brief account of ECICE and its adaption to Antarctic ...” Would it be more correct with “brief account of ECICE, implemented correction schemes, and the adaption to Antarctic ...”

Yes, of course — we have corrected this.

L78: remove “first”?
L78-79: since the entire record is never presented, I would suggest deleting the period. Simply “In sec 3, the outcome of the Antarctic sea ice type concentration mapping is compared with ...”

We have rewritten the sentence as: "In Section 3, results of the Antarctic sea ice type concentration mapping are compared with results from..."

L91: Any reason why SSM/I can be included but not SSMIS (and other passive microwave radiometers)? Why mention SSM/I if it is not included in the present method/production?

After 2002, the preferred satellite instruments to be used are AMSR-E and AMSR2 as they have higher resolution than SSM/I and SSMIS. SSM/I was mentioned as it can extend the record backwards before the AMSR-E era. SSMIS can, of course also be used, and actually it can close the gap between AMSR-E (until Oct 2011) and AMSR2 (from July 2012). We have mentioned this here (beginning of section 2.1).

L100: “Most methods” please include references.

We have included the reference to the ice concentration algorithm comparison paper by Ivanova et al., (2015), this saves specifying 10 extra references.

L132: “If MYI .. drops at any location during a warm spell ...”. Are there not any restrictions on this drop to occur in the vicinity of where the warm anomaly appears?

We actually meant "at any location affected by the warm spell" and and have corrected the text accordingly.

L140: “After that, MYI can only drift ...” What exactly do you mean by this? If you mean that no more MYI will be created (per theoretical definition) after this point, then say this more clearly. In the same sentence is used the word “melting” which is not a part of “drifting”...

Please re-phrase this sentence.

We have slightly reworded the sentence: "After that, during the cold season, no new MYI can be generated. MYI can then only drift, and its concentration can only be changed by divergence, convergence, and melting."

L144: "... boundary of MYI cover ..." Please define the boundary of MYI cover. Is this where MYI conc = 0%?

We use a threshold of 20% MYI concentration to define the MYI boundary. We have inserted this information in the manuscript (now L171).

L153: "... sudden reductions ...", sudden reductions in time, I assume?

Yes, we use "sudden" in the standard, temporal, meaning, as the specification "(within one day)" suggests.

L156: "The values of the parameters ...". Please indicate from where these values are taken, e.g. include reference or discussion on how they have been chosen.

The values were empirically determined for the correction schemes in the Arctic and have been kept here. This is explained in Section 2.3. where we now also state why we have kept the Arctic values.

L160: Why is AMSR-E presented here when the ice type record covers 2013-2019?

AMSR-E data will be used in the next step. However, using them has already been implemented.

L167-168: Is the Melsheimer reference the right reference to add just after NSIDC?

Yes, it is, as the NSIDC grid in the context of sea ice type retrieval is described in more detail there.

L168-169: Could you please elaborate a bit more on this, e.g. by simply presenting the approximate spatial resolution of the used input data?

Yes, more explicit information has been inserted (section 2.3, below equ.(2)).

L174: As "ASI" is used only once, I suggest to just fully write the full name here, for easier readabilty.

This is why we have put the full name and the link and reference into a footnote.

L174-175: It is a bit unclear from what seasons the training data is collected. Is MYI data collected from only beginning of the freezing period. Please give a bit more details.

The MYI data are collected from the first months of the freezing season. There is now a new appendix (Appendix A) that gives details about the sample data.

L188: Are there any reasons for using ERA Interim instead of the newer ERA5?

When most of the presented work was done, ERA-5 was not available yet.

L190: When mentioning the potential NSIDC ice drift data, please include a comment on why OSI SAF ice drift data are chosen to be used, and whether NSIDC data have been tested out in the ice type retrieval. Also, please note that an ice drift climate data record from OSI SAF is in the pipeline for this spring 2022 (regarding L202-203).

NSIDC drift data have been used already for retrieval in the Arctic (see Ye at al., 2016b). At the time this study was started, the used OSISAF data seemed the best choice, but we are actually considering switching. This has been included in the text (L247, and at end of Summary/Conclusion)

L191-192: this is a repetion

Well, we once more reference Tables 2 and 3 (after referencing them in sections 2.2.1 and 2.2.2) and see no harm in that.

L218: Are these threshold procentages randomely chosen, or can you comment on why 50% is used for YI and 70% for the others.

They were empirically chosen.

L225: To my knowledge, NIS do not produce Stage of Development maps. Plese check this out.

NIS of the Norwegian Met. Institute is at least one of the three partners of that project. As far as we know, NIS has contributed some regional ice charts (but not SoD?). We have now left out NMI/NIS

L225: To my knowledge, no SoD maps are available on the webpage in 2014. Please chech this out.

Thank you for pointing this out, concentration maps start in December 2014, but SoD maps only in May 2015. We have corrected that.

L231-233: Is this information relevant (and is it true in practice?)?

We considered this information necessary because it makes some MYI (in the first half of the season, until end of June) appear in similar colours as FYI.

L235-236: when you write “an overall correspondence” - is this referring to results shown here, or did you check all available charts against the product. Please give more information on what is in overall correspondence and how this has been concluded.

We have compared at least one SoD chart per month (there is one per week) for two entire seasons.We consider publishing these comparisons as supplementary data. We will elaborate on that.

L279-280: The contribution from Ex-MYI is not shown or taken into account here. Please include this contribution to the discussion of FYI and YI.

As already mentioned above, we had decided against showing Ex-MYI (now called non-MYI, see above) as we have not investigated it thoroughly. We can only say that it is actually FYI or YI (but cannot tell which).We have added this information to the text where this is first discussed in section 2.2.2

L310: Please add affiliation or title for Ted.

Yes.

L312-314: This paragraph is unclear. Please re-phrase.

Yes, we have tried to rephrase and improve it (now L394ff.).

L315-316: I suggest that you include some mapping examples from September/October to better visualize for the reader why you see an increase in the MYI area.

In L315-316, we speak of a decline of MYI concentration, not an increase.

L332: “spatial resolution” or grid resolution?

Yes. it should be "grid resolution".

L334: “(so far the only source of ice type information in the Antarctic)”. This should be reworded. I assume that in situ data exists to some extent. Also, OSISAF ice type product exists and is processed on an operational basis.

We meant detailed and comprehensive ice type information (this OSISAF ice product has only FYI and MYI) that does not only rely on automatic satellite data processing. we have reworded to *“(so far the only source of detailed ice type information in the Antarctic, apart from ship-based observations of the ice conditions)”*, now L444.

L341-342: “The data become unstable toward the end of the freezing season in September/October, with MYI being underestimated” I think this has not been shown clearly in the result section. And how do you conclude that MYI is being underestimated?

See L315-322 (now L400ff.) in the Discussion section: We retrieve much less MYI than the SoD charts which, however, seem a bit "unstable" or inconsistent. In the discussion, we give a possible physical explanation for an underestimation (now L404-407). Here, and in the Summary/Conclusion (L453), we have added a "probably" before "underestimated".

L344: I would remove the sentence “outweighs the shortcomings”. This only put your product in a bad light I would say.

Thank you for this encouragement! We have removed these words, but added a "preliminary" earlier in this sentence.

L346-350: Give better and more correct references to upcoming satellites/sensors. And please add a comment on why 1.4 - 36.5 GHz is assumed to make scatterometer less important

We have revised these lines and added references (end of Summary/Conclusion), and also indicated why using CIMR’s 1.4 GHz channels is useful for MYI retrieval.

Response to RC3:

Note that we quote the reviewer's comments and suggestions in red.

51: One reason why most MYI is in the Weddell Sea is that the gyre that transports MYI away from the coast to the north and northwest also transports in ice from the north and northeast. This is seasonal ice that gets transport into the Weddell, where it compacts along the ice shelve and Antarctic Peninsula and, along with less solar insolation and colder temperatures, allows that FYI to survive into MYI. This seems a salient point to make here as it is the mechanism to form MYI.

We have added a statement on that in the text: “In turn, seasonal ice is transported into the Weddell Sea from the north and northeast, can be pressed and compacted against the ice shelves and the coast of the Antarctic peninsula where it survives the summer ad becomes MYI.” (now L56ff.)

90-92: No SSMIS sensor data are used?

After 2002, the preferred satellite instruments to be used are AMSR-E and AMSR2 as they have higher resolution than SSM/I and SSMIS. SSM/I was mentioned as it can extend the record backwards before the AMSR-E era. SSMIS can, of course also be used, and actually it can close the gap between AMSR-E (until Oct 2011) and AMSR2 (from July 2012). We have mentioned this here (beginning of section 2.1).

174: How is the “beginning of the cold season” defined? Is it the minimum total extent? But at the minimum, there may be regional gains and regional ice losses occurring (the minimum marks when the gains start to outpace the losses). Ideally, you would use the minimum at given grid cell or at least regionally.

It is not feasible to define the beginning of the cold season grid-cell-wise (using reanalysis data), in particular as the drift correction is not grid-cell-wise but rather a neighborhood operation. This would also cause problems at the region boundaries if the beginning or the cold season were defined region--wise. Since we are here identifying sample areas for FYI and MYI, looking for the beginning of regrowing ice regionally is a reasonable approach. We have included this information now in a footnote.

187: How accurate are the ECMWF 2 m temperatures over the sea ice? There are several coastal stations that I assume provide observations, but over the sea ice, the observations are quite sparse, with few buoys (compared to the Arctic). It is reasonable to use ECMWF as that is what is available and better than nothing. But I think a mention on potential uncertainty is worthwhile here.

Yes, we have mentioned this (now L241ff.).

188-189: And likewise for the ice motions. Antarctic motions typically have higher errors because of the variability of the ice (flooding ice, etc.) and lack of buoy validation. Again, don’t need to go into great detail, but a comment on the uncertainty would be helpful.

Here as well, we have mentioned this.

234-235: In what format are the SoD charts provided? It seems they are used here merely qualitatively. If they are just images, that makes sense. But if they are in some sort of data format (e.g., GeoTIFF), they could be used to do some quantitative comparison with the ECICE. And also, as noted below, they could be manipulated to consolidate the different ice classes into the main three with a clear color scale to more easily visually compare with ECICE.

We have used the maps (graphics files, PNG). Analysing this in more detail using the data in original SIGRID3 format is planned but would probably beyond the scope of this paper.

236, Figure 2: This figure seems a bit odd and confusing to me. It seems like there are two SAR images overlaid on the ECICE image. But they overlay, so block the ECICE. Once can see some continuity, so the performance looks reasonable, but it seems odd to show only one figure with one or the other (SAR or ECICE). The ECICE color scale seems to have several more gradations than the 5 indicated in the legend. The legend color scale should match the colors plotted. It seems like creating a two-panel image – one with the ECICE and one with the SAR images and then overlay the contours on both – would be clearer?

(Note: now Figure 2) We have considered modifying the figure, splitting it into two panels. However, this figure is only to illustrate an episodic check of our first results and we want to keep it compact. Note that the legend gives only the most important color shades (the idea was not to clutter the plot with too much information), we now give a hint about that in the figure caption..

239, Figure 3: I guess it is okay to have the SoD color scale in the Appendix – at least the authors acknowledge that it isn't legible in the figure. But ideally, a better color scale would be included/added to the figure. And it's clear that the SoD figure has more categories than the ECICE, so it is a bit hard to directly compare, though the overall patterns are clear. It would be more work, but if it were possible to actually take the SoD and create a custom plot with the SoD categories combined into the three ECICE categories, that would be quite helpful.
267, Figure 5: As for Figure 3, it would be nice to have SoD in a simplified form with all types consolidated into the three ECICE types and with a color scale legend provided with the figure.

(Note: now Figure 4) As mentioned above, using the original data might be beyond the scope of this paper. Therefore, we have decided to keep the figure as is, in particular as there is already one "color family" for each of the three ice types: pink/purple hues for YI, yellow/green hues for FYI, brown hues for MYI.

Minor Comments (by line number):

45: I've seen "snow-ice" with a dash to connect the two nouns and denote a unique type. But this is perhaps simply more of an editorial/style decision.

We use the convention to write a two-word compound without a hyphen, just like "sea ice".

114: Typo, "cost" not "coast"

Yes.

174: Not sure why the ASI reference is given as a footnote? If that is The Cryosphere style guideline, I guess that's okay, but in my view, datasets should generally be cited as regular references.
(text)

We wanted to give the direct URL for the data on our server and also the reference to the PANGAEA data set, and avoid lengthy parentheses.

346: It seems like the chart color legend (Table A1) should be after the beginning of the Appendix text? But as noted, it would be helpful to create a new legend that combines the relevant classes into the three main types for the figures in the main text of the manuscript.

Figure placement has to be straightened in the final version any way...