

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-381

Anonymous Referee #1

Referee comment on "First results of Antarctic sea ice type retrieval from active and passive microwave remote sensing data" by Christian Melsheimer et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-381-RC1>, 2022

Review of

Antarctic sea ice types from active and passive microwave remote sensing

by

Melsheimer, C., et al.

Summary: Little is known about the distribution of different sea-ice types of the Southern Ocean, while of the Arctic Ocean sea ice types several satellite data products exist, comprising binary products, partial concentration products, or sea-ice age. This study aims to contribute filling this knowledge gap by means of an empirical approach combining active and passive microwave satellite observations of the years 2013 through 2019. This approach, called ECICE, has been developed for Arctic sea-ice conditions. In this study it is applied with minimum modification to Antarctic sea-ice conditions. Multiyear ice partial concentration corrections developed for the Arctic Ocean are as well applied to Antarctic sea-ice conditions with almost no modification. A few selected examples of obtained ice-type concentration maps are shown and discussed in comparison to other satellite remote sensing products in an exclusively qualitative manner.

General Comments:

I find the goal of (improved) understanding and quantification of the different Antarctic sea-ice types timely, highly relevant, and important

However, I am not convinced that this manuscript can be seen as a substantial contribution towards accomplishing this goal. It does not make a significant contribution in this research field.

I provide arguments for my decision in the general comments that follow which the authors' please see as umbrellas for the numerous specific and detailed comments provided further below.

GC1: Physics

Arctic and Antarctic sea ice differ fundamentally in the physical properties and hence microwave signatures that are required to provide a reliable, credible, and accurate estimate of the distribution of Antarctic sea-ice types. The paper (and possibly the work behind) fails to adequately describe and discuss these differences. Examples are: 1) Arctic multiyear ice differs from Antarctic multiyear ice in its development history - aka how summer melt changes the sea ice characteristics, its vertical structure and its microwave signature. 2) Antarctic sea ice is experiencing an enhanced atmospheric influence year-round by a considerably larger amount of low pressure systems that regularly cross the entire sea-ice cover, thanks to the geographic setting of the Antarctic sea ice. This influence creates a substantially more dynamic environment for sea-ice growth and different forms of deformation. In particular is sea-ice formation along the ice edge governed by the so-called pancake ice cycle. 3) The snow cover on Antarctic sea ice is known to be much more variable in terms of its depth, composition, (density) layering, and interaction with the underlying sea ice in response to issues mentioned in 2).

Each of these three explicitly mentioned differences in the sea-ice physical properties have an influence on the microwave signature of the sea ice that needs to be understood and adequately quantified before a sea-ice type retrieval algorithm based on satellite microwave observations can be developed or an existing approach such as ECICE can be modified so that it is fit for purpose - aka Antarctic sea ice type concentration retrieval.

GC2: Previous work / References

The approach and the results are potentially of high relevance for the scientific community. So far, this manuscript does not really convince with arguments that the modelling community would benefit from knowing where Antarctic sea ice is of which type. More importantly, however, since this manuscript (and the work behind) arguably is one of the first attempts to derive Antarctic sea-ice type concentrations from combined active / passive microwave satellite data, it has to be embedded substantially better into the overall context. This refers to 1) Discussion of existing (binary) sea-ice type retrieval approaches (for the Arctic) and why (possibly) none of these has been applied to Antarctic sea ice. 2) Discussion of our current specific knowledge about the Antarctic sea ice type

distribution and microwave signatures - aka: typical emissivities / brightness temperatures / radar backscatter values for the surface types encountered - including snow. Here the paper, to my opinion, fails to provide the required background to further develop / modify ECICE and to adequately interpret and discuss the results obtained.

GC3: Description of the methodology

The description of the methodology fails to adequately motivate the steps carried out - especially in light of the differences between Arctic and Antarctic sea ice (see GC1). It is not clear how the distributions of brightness temperatures and radar backscatter values were actually obtained. These values are not backed up with values from the literature / other studies (see GC2). The description of input data preparation is not complete.

GC4: Description and interpretation of the results

The description and interpretation of the results mimics the lack of background information about the physical properties and the related microwave signatures - as well as the general differences between Arctic and Antarctic sea ice in terms of its temporal behavior and the role snow has.

Clearly, the paper would benefit from including an Antarctic expert and an expert for snow on Antarctic sea ice. This appears to be mandatory to appropriately interpret and discuss the results qualitatively and quantitatively - be it the multiyear ice area time series, the spurious multiyear ice in the northwestern Ross Sea, the usefulness of the two Arctic multiyear ice concentration correction approaches under Antarctic conditions, or, in general, the likelihood that there is ample opportunity for Antarctic young ice and first-year ice microwave signatures to leak over into the one of multiyear ice - plus an understanding about where icebergs' microwave signatures belong to. Another issue that is completely missing in these descriptions is an investigation about the total concentration the partial ice-type concentrations sum up to. There is multiple evidence in the results shown, that this sum is not 100% but the actual total sea-ice concentration which accuracy is not further discussed and/or shown in comparison to independent data either.

Finally, the authors only aimed for a qualitative evaluation of their results, so this is all they included in this paper. Fine. However, the description of the data used for the evaluation is not complete to understand the scope and relevance of the authors' efforts. The results of the evaluation are very vague, leaving a user in doubt about regions and time periods the data are credible. It seems questionable whether presenting such results has the effect the authors potentially hope for - aka: an encouragement to continue their work and plenty of users of their data set. How can it be ensured that the results taken up by users are not misleading them?

Specific Comments:

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.

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

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.

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.

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. ?

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.

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

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.

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 interpret 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.

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.

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?

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)?

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

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.

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 referring 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 referring to melt-refreeze cycles or the like and need to specify this here to avoid confusion.

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"?

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 σ_{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.

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:

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?

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

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

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

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

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?

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

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.

Table 2:

- 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.

- 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).

- 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.

- 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.

Table 3:

- I note that the choice of the values for these parameters specifically for Antarctic conditions has not been discussed and/or motivated 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.

L187-191: Three comments here:

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?

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).

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?

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

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.

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

2) What is the statistics behind the data? What is the time period? At how many data per

surface type do we look?

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).

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

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

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.

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.

Section 3.1:

- 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]?

- 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?

- You decided to provide a qualitative intercomparison without computing radar backscatter (σ_{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?

- 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 into account the specifics of seasonal changes in microwave signatures in the Antarctic compared to the Arctic.

- 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 rephrase 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.

- 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 be interesting to see an example of this - perhaps in the appendix or in supplementary material.

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

Section 3.2:

- 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?

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

- 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?

- 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?

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

- 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?

- 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.

- 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.

Figure 3:

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

- 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.).

- 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 a more recent and hence more accurate land mask.

- 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

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.

Section 3.3:

- 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.

- 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.

- 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?

Figures 6 through 9:

- 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 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?

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

- 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?

- 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.

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.

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.

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.

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%.

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."

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

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?

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?

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.

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.

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.

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, wetting 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.

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 surprising.

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

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

Typos / Editorial 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.

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."