The Cryosphere Discussions

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Comment on tc-2022-95

Anonymous Referee #1

Referee comment on "Inter-comparison and evaluation of Arctic sea ice type products" by Yufang Ye et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-95-RC1, 2022

Review of "Inter-comparison and evaluation of Arctic sea ice type products", by Ye et al.

Summary

This paper compares different sea ice type products currently available to the community. The products are based on passive microwave data, scatterometer data (C or Ku band), or a combination of both. The products have been developed empirically via training data. The type fields are inter-compared and evaluated against a widely-used sea ice age product and SAR retrievals. The products perform better in mid-winter than in early or late winter when melt/re-freeze may occur. Ku-band scatterometer generally is better at type discrimination. Combination of passive microwave and scatterometer data can yield better performance at times, but not in all situations.

General comment

This is a fairly comprehensive review of the primary sea ice type products available. There are notable differences in how the products are assembled, the input source data, and their performance in different conditions. Thus, this paper is a valuable contribution to the community be providing such an assessment. The paper is quite thorough and overall it does a good job in presenting the inter-comparison and evaluation of the products.

Specific comments are below, but one overall comment is on the SAR data used for evaluation. In general, SAR is going to be the best "truth" for comparison. It is high resolution, so it can delineated even individual floes often. And it is all-sky, so retrievals of type are available anywhere the sensor collects imagery. However, the challenge with SAR is interpreting the imagery. The authors interpret the SAR imagery and classify various

locations as a given ice type, but they don't give a particular rationale or provide references for their classification basis. Often, expert ice analysts interpret SAR fields for operational ice charts. They have deep experience in understanding the imagery and properly defining features. It appears the authors here classify the imagery themselves. This is okay, but I would like to see more substantial justification for their classification.

Another weakness with the SAR comparison is that it is just a few scenes in selected regions and selected periods. And even within the SAR scenes, a few specific locations are picked out as "pure types" for comparison. Ideally, a full SAR image would be classified and compared. I know automated SAR classification algorithms for sea ice are troublesome, so I can understand the approach taken, but it results is a fairly ad hoc and qualitative evaluation. Since this paper is otherwise quite comprehensive, I won't request more evaluation, but ideally (perhaps in a future paper), it would be good to get classified SAR images – perhaps from an expert ice analyst at an operational ice center – and conduct a more comprehensive and quantitative evaluation of the ice type products.

A final note is that there is a need for a thorough copy edit for English language style and grammar. The issues are mostly minor – in particular, there are numerous missing articles ("the", "a", "an") – but they are widespread throughout the manuscript. I don't bother to point them out individually as they are too numerous, but they need to be addressed before final publication.

I recommend publication after minor revisions.

Specific comments (by line number):

11: The authors definite "sea ice type" as "SIT" here. This is fine and it is used consistently throughout the manuscript. However, as a sea ice scientist, "SIT" means "sea ice thickness" to me. And particularly with numerous thickness products coming out from altimeters, "SIT" is becoming quite common in the community to denote thickness. I can understand wanting to use an abbreviation and "SIT" makes sense for ice type, and the context is clear throughout the manuscript. So, I can't say it needs to be changed, but it might be something for the authors to consider. For me, every time I saw it, "thickness" popped into my mind first until I recalibrated. I can't think of another good abbreviation myself, but one could just use "type" or "Type" as a short-hand, instead of "SIT".

28-30: I'm struck by the use of more than author listed and then "et al." in the citations – i.e., "Comiso, Parkinson, et al., 2008". Generally, if there are more than two authors, just the first author is listed followed by "et al." – i.e., it would be "Comiso et al., 2008". In looking at The Cryosphere guidance for citations, I don't see anything that indicates two

authors should be listed, so I'm not sure of the rationale. This seems to be done throughout the manuscript. (If there are only two authors, you list both, e.g., if it were "Comiso and Parkingson, 2008".) Not a big deal and I assume the copy editing will decide the proper citation format. I just haven't seen this before and it struck me as odd.

31-32: Be careful about terminology. "Thin" and "Young" ice are standard stage of development classifications. I think here you mean "thinner and younger" for FYI, and then "thicker MYI". I'm also not sure what you mean by "firm" in relation to MYI?

57: "ergodic" is an obscure word – I was not familiar with it. Based on my understanding after looking it up, I'm not sure it is used properly here. Regardless, I think a simpler word is appropriated here or I wonder if it is needed at all – "combined use of both data" is clear to me.

62-63: "While ice chart…" is a confusing sentence – not sure what it is say. I would suggest revising.

72: Just one example of grammar/style issues: "…are detailed investigated." – It should be "are investigated in detail."

107-109: Is AMSR-E used in the product? The description indicates only AMSR2 is used. So, why describe AMSR-E characteristics? Why not just describe AMSR2 characteristics?

109: Maybe another grammar/style issue: "working" is okay, but typically when describing sensors or satellites, "operating" or "collecting data" are more common. "working" seems a bit colloquial here.

147: This goes for all products, but noting here because NSIDC products have specified references that should be used. For SIA, it is:

Tschudi, M., W. N. Meier, J. S. Stewart, C. Fowler, and J. Maslanik. 2019. *EASE-Grid Sea Ice Age, Version 4*. [Indicate subset used]. Boulder, Colorado USA. NASA National Snow and Ice Data Center Distributed Active Archive Center.
doi: https://doi.org/10.5067/UTAV7490FEPB. [Date Accessed].

This should be cited in the manuscript text and listed in the references. I see that the dataset website is noted in the Acknowledgment section, but where a reference is provided, it should be included in the manuscript proper, including the dataset DOI. I

know all datasets do not provide a formal citation and/or DOI – for example for OSI-SAF, their recommended citation is simply: "The type dataset shall be referred to as the *Sea ice type product of the EUMETSAT Ocean and Sea Ice Satellite Application Facility (OSI SAF, osi-saf.eumetsat.int)*." If that is all that is provided, that is fine, though I would also say that the product ID (OSI-403-d) and version (if provided) should be included. The other datasets used should be cited to the extent they properly can be.

185-186: I think the potential for MYI increase could be explained better here. In practice, overall Arctic MYI cannot increase over the winter – it can only decrease via advection out of the Arctic. "Temporary" increases can happen within products due to divergence – e.g., a 100% MYI pixel diverging into two pixels with 50% ice each; if the threshold for detection is <50%, there will now be two pixels. And regionally, MYI can increase, both due to divergence or due to advection into the region from neighboring regions.

191: This is discussed a bit more later, but this left me hanging: "why such a dramatic peak in the first half of winter?" Maybe provide a brief explanation and then say it will be discussed further later in the paper.

204: I would use "to" instead of a "-" because it looks like a minus sign. Or use an "em-dash" or "en-dash" with spaces on each side.

219: Figure 5 is mentioned quite cursorily here, but I notice the behavior of several products in BS during 2016-2017. That sticks out compared to other years and regions. Why was the performance so different?

224-225: Okay, the KNMI-SIT increase is mainly in the BS and ESS regions. But why? In general, this paragraph (223-229) feels like it needs to drill down a bit more and give more detail/explanation.

259: Kind of the same thing here. Okay, you have an overestimation of MYI, but that doesn't specifically explain the abnormal increase in MYI during 2016-2017. Why was the MYI overestimated in the one year versus others.

265: How are cases selected? Were they ad hoc? Random? Was it simply availability of imagery? Or was there some physical rationale to select the scenes? I understand in general wanting different regimes and different time periods, but why those specific images on those specific days at those specific regions? In other words, what "different conditions" were you selecting for here?

268-271: Following from my general comment above, how were characteristics of the SAR

images used for visual interpretation. What is the basis? There are no references here to justify the classifications

273-278: This paragraph illuminates the previous comment. The text is very "squishy". You say things like "appears to be MYI", "more likely to be MYI", "which could be interpreted as newly generated FYI". This is very qualitative and seemingly tentative. I think maybe you just need to say why something "appears to be MYI" – what is in the SAR image that leads to that conclusion and what is the basis for that?

288-293: Same here. You have "could be identified as MYI", "are a typical feature of FYI".

299: I guess there is a thematic reason for the order – looking at early and late winter as "edge" cases, but it seems more logical to order these subsections chronologically: early winter, mid-winter, and then late winter.

315-319: Again, very qualitative.

391-446: I can see the logic of discussing the methods here – you are linking them to the performance assessed previously. However, to a large extent, this feels like it should go with the data product descriptions in Section 2.1. I guess moving this there would make that section rather long. But I kind of feel like I get to here and I finally understand how the type products are created – after all of the evaluation and comparison. I'll leave it to the authors to decide if this fits better in 2.1 or should stay here. Or maybe, put some description in 2.1 and then the relation to the product evaluation here.

400: "[55]"? Is this a numbered reference?

434-436: Melt affects the performance in early and late winter. But melt also basically makes the algorithms ineffective in spring and summer. That is implied, but never really stated explicitly it seems.

Figure 5: Following up from above, I'm struck by the notable increase in many products in BS during 2016-2017. That is not noticeable in another region or year. There is some discussion, in relation to OSI-SAF in Figure 7, but the text doesn't really discuss this. I think this deserves some explanation in the text.

Figure 5: What is the shaded green region that accompanies OSISAF? Maybe it is in the main text and I missed it, but regardless, it should also be included in the caption for the

figure.

Figure 6-7: This is a style/aesthetics thing, but the beige/brown for the OW seems odd. Such a color is more commonly used for land, and definitely not for ocean. I would suggest considering a different OW color – just swapping the land color (light gray) for the ocean color, would be more logical to me. Of course, you want to make sure that the colors contrast and are clearly delineated. But a good solution other than beige/brown for OW seems possible.