

## ***Interactive comment on “The EUMETSAT sea ice climate record” by R. T. Tonboe et al.***

**W. Meier (Referee)**

walt.meier@nasa.gov

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### Summary

This manuscript documents the new EUMETSAT sea ice climate record, including the processing, quality control methods, and estimation of uncertainty. Results of the product are presented as trends in extent and, concentration, and days of open water. These results are consistent with similar analyses using other sea ice products, showing strong declines in the Arctic, and weaker increases in the Antarctic.

### General Comment

The data product presented is a well-constructed product that will be beneficial to the community. Overall, this manuscript describes the product well and demonstrates its utility for monitoring climate change and variability. There are a few sections that could use some clarification and/or further details, which are noted below. The most signifi-

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cant comment is that there are places where the text is quite repetitive – as if sections were written independently and then appended together; examples are given below. Also the writing style is somewhat unimaginative, making it read rather dull. The main example here is that nearly every figure is introduced in the text in paragraphs starting “Figure X shows...” or “Figure X is showing...” These read more like figure captions instead of a discussion of results. I would recommend rewriting many (if not all) of these to be more conversation. For example, instead of writing “Figure X shows Y has an increasing trend”, simply write: “Y has an increasing trend (Figure X)” and then carry on the discussion from there.

One more issue is the section breakdowns. There are places where more nested subsections should be used. For example, Section 2.5 is “The sea ice concentration uncertainties”, then Section 2.6 is “First component: instrument noise, algorithm and tie-point uncertainties”. This is clearly a subsection under 2.5, so it shouldn’t be listed at the same level, 2.6, but rather at something like 2.5.1 or 2.5a. I’m not sure of the specific style guidelines of The Cryosphere – maybe it doesn’t allow more than one nesting of subsections? But even if so, I would then break out the contents until un-numbered sections under the main subsection, and denote them by underlines and/or italics.

Finally, some of the figures could be a bit more polished. Fore example, the legends in Figures 4-9 look more like working figures. Maybe do “Total Bias” instead of “total\_bias”. Also the x-axis labels are a bit messy – labels and ticks at regular intervals would be better – e.g., at the beginning of a month, not varying days within months; particularly for Figures 4-7, where the date labels seem rather random. And the y-axis labels in Figure 8 should be outside the axis, not inside Also, remove the straight lines across missing data in Figures 6 and 7. There is also inconsistent marker styles (line vs. no-line) in Figures 8 and 9. Finally, the fonts for Figures 4-9 could be a little nicer – they look almost dot-matrix printed.

Much of these suggestions are style rather than science, but I still think making the

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suggested changes will help the manuscript convey the science better. These shouldn't be too difficult to accomplish; my decision is Minor Revision.

Specific Comments (by page/line number):

1/30: There are several other, more recent papers on the increasing Antarctic sea ice, e.g. Goose and Zunz, *The Cryosphere*, 2014; Holland, *GRL*, 2014; Li and Holland, *Nature*, 2014; Holland and Kwok, *Nature Geoscience*, 2012.

2/1-2: It says the extent is defined by concentration >15% as in Parkinson and Cavalieri (2008), but later a 30% threshold is used. The text should be made consistent.

3/9: What are "Dicke" radiometers? Not sure this detail is necessary.

3/14: Explain why SMMR data are produced only every other day.

3/23: RSS is now distributing Version 7. Understand that Version 6 was used here, but would be good to mention the new version. And will Version 7 eventually be used (e.g., a future reprocessing)?

4/4: Where is the SSMIS L2B NRT data stream obtained?

4/22: Units for "220" and "60"?

4/23-24: The approach of using 100% and 0% tiepoints for brightness is not uncommon for visible imagery. However, there is potential error in that new ice growth will be darker, even at 100% concentration. So the tiepoint approach will underestimate new ice. This is a relatively small area (especially during spring-fall when there is visible light), so I don't think it affects the analysis significantly, but would be worth mentioning.

4/29: I believe the official designation should be "U.S. National Ice Center"

5/1: Where were the charts obtained – directly from NIC? NSIDC also has a database of gridded charts (<http://nsidc.org/data/g02172>), so it's good to be precise on the data citation. And while these are operational charts, there are a couple (older) peer-

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reviewed journal articles that could be cited: Dedrick et al., Canadian J. Remote Sensing, 2001; Partington et al., JGR, 2003.

5/1-20: This is an area where the text is repetitive and could be reorganized a bit to be more concise. For example, on line 1, it says the charts are produced on “a regular basis”, then on line 17, it says “a weekly compilation” – why not just say they’re produced on a “weekly basis” on line 1 and eliminate the later sentence. Similarly, lines 7-8 say the source is “primarily satellite imagery”, then in lines 11-17 the specific satellites are used and the caveat that ship and aircraft observations are also used.

5/20: Missing, or at least not clear, is that the chart concentrations are given in ranges of 10ths. This is particularly important because the PM concentrations are to the nearest %. It’s not really explained how this is handled and how it affects the comparisons. For NIC values, is the midpoint used as the concentration (e.g., for 7-8 tenths, is a value of 75% used for comparison), or are the PM values binned into the NIC increments?

8/10-28: Another place with the section nesting doesn’t seem quite right. Maybe just remove the 2.4 designation and merge with 2.3, which is very short. There is also a lot of repetition of text in the two sections – for example, line 5 is exactly repeated in line 28. Also, lines 11-14 (and page 9, lines 1-2) repeat much of the text in 2.3.

9/28 – 10/1-3: repeats 9/21-22.

10/6: Section 2.7 is really part of the error in 2.6, right (i.e., the “First component”)? Then it shouldn’t really be a separate section at the same level as 2.6 (and the following “Second component”).

10/26: Again, isn’t smearing (2.9) part of the representativeness error (2.8)?

12/14: What are the monthly varying ocean masks? Where are they from, how are they used? This is later explained in 2.13 (I think) but the explanation should be put with or before its first use here.

13/29-30: the land-spillover correction by Cavalieri et al. (1999) is a bit more involved

than described and includes not just grid cells along the coast (i.e., directly adjacent to a land grid cell), but also grid cells one and two pixels away from the coast, with different corrections applied to each. It's okay to leave the details to the reference, but as it's written now, the description is somewhat misleading.

14/11: I think you mean "Grid cells with missing data are filled with interpolated values only in the level 4 processing..." As it reads, it sounds as if one might consider interpolating and filling cells with data.

14/12: "Daily coverage is never complete..." – is this due to the pole hole? Even so it doesn't really go with the rest of the sentence "...and occasionally there are missing scan lines..." Maybe "Daily data coverage is never complete due to the hole near the North Pole and occasionally there are missing scan lines..."

14/16 – 15/12: This interpolation procedure description seems unnecessarily complex and difficult to follow, and some parameters in the equation are not defined (e.g., what are "W" and "w"). It's okay to show the specific equations, but there should also be a clear text explanation the approach (e.g., similar to what is done for the blending of Bootstrap and Bristol concentration – the equations are given, but it also simply says a weighted average with equal weighting at 40% concentration).

15/20: Suggest removing "It is clear".

15/26: repeat of 15/16

15/24: Here is where the issue of the NIC use of 10ths bins is salient – how is this handled? Also, the charting changed quite a bit when Radarsat-1 started to be used in 1995 – the charts got less dependent on passive microwave, more detailed, and probably more accurate. So I think pre-RS1 and post-RS1 might have quite different characteristics in comparisons with the PM data. Was this looked at? I don't see anything obvious in Figure 4, but I would expect some difference.

16/8-9: Ocean cells have above 0 concentrations even with the atmospheric correc-

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tions? What about the smearing error – couldn't that contribute by smearing out the ice into open water areas near the ice edge?

16/30: National Ice Center should be capitalized.

17/1: The missing chart period is noted for (at least) the third time. It's okay to reiterate the point for clarity, but each time it's written as if for the first time. At least rewrite to "As noted earlier, there were no digital ice charts available..." But I don't think it's needed at all here.

17/18: The overlap is during July and August, which isn't during the minimum. And I don't think the limited number of ice data points is the main factor here – it's that July and August is a time of significant melt and melt pond coverage.

17/30: Repetitive of earlier description of the NIC charts – don't need to say it again here.

18/2: Who are the "two other producers"? Is this referring to other ice charting centers?

18/10-11: "...yield the same concentrations..."

18/30: Why is the 30% concentration used to define extent? It's perfectly reasonable, but why use that instead of the more common 15% threshold?

19/16: The 5% and 95% ranges need to be explained. I would assume they describe the range between the highest and lowest 5% values. But then how in Figure 10 is the 2012 extent within the 5% contour when it is the lowest extent in the record?

20/1: Recommend citing Parkinson, GRL, 2014, who calculated open water season trends for the Arctic from passive microwave sea ice fields.

20/7-10: This sentence is a bit unclear. I think I understand – different periods were tested and 5 days was the shortest period found that gave reasonably stable values without much noise. Right?

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21/23-29: This seems odd to me. In Figure 14, the open water trend near the edge is +30 days for 2004-2014, but there have been record high extents during the same period? I guess I can see larger extents for shorter periods, but the large number seems unlikely – the open water season is over a month longer, but still ice reaches record maximums? Maybe this mostly points to the shortness of the period. Maybe it's just one year (or two) that are really anomalous that dominate the signal?

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