The Cryosphere

Open Access

EGU

Discussions

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

R. T. Tonboe et al.

rtt@dmi.dk

Received and published: 17 June 2016

Thanks for the constructive comments to our MS and helping us making this a better paper. We have tried to accommodate nearly all. We have reprocessed the ESICR and ice chart comparison using the published NIC dataset at NSIDC and NIC as you have suggested. See specific answers in the text below.

Interactive comment on "The EUMETSAT sea ice climate record" by R. T. Tonboe et al.

W. Meier (Referee) walt.meier@nasa.gov Received and published: 22 April 2016 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, show-

ing 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 significant 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. We have tried to follow you guidelines on style.

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. We have restructured section 2 with subsections as you suggest.

Finally, some of the figures could be a bit more polished. For example, the legends in Figures 4-9 look more like working figures. Maybe do "Total Bias" instead of "to-tal_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 is 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 low almost dot-matrix printed. You are right! These figures have been replotted and the ice chart comparison reprocessed using publicly available and documented data sets. The period has been extended to cover the entire dataset period (except the ice chart data gab on the southern hemisphere 1994-2006).

We have left out the "total" bias and std from the plots. The quantity was dependent on the number of ice and water points and so not so meaningful.

Much of these suggestions are style rather than science, but I still think making the 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. Thanks. We have updated the references.

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. We agree this is inconsistent! The threshold used in this paper is 30%. This threshold was selected for the example applications of the dataset in section 3.4 because it gave the most stable and least noisy results. That the threshold is higher than in Parkinson and Cavalieri (2008) maybe because we are not using weather-filters in the processing and occasionally there is open water noise over 15% (as shown in the ice chart comparison).

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

3/14: Explain why SMMR data are produced only every other day. Ok, it is included in the text.

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)? Ok. It has been included in the text. The next reprocessing will be done using the EUMETSAT climate SAF dataset, even though there are still some open issues about the geolocation correction to be resolved.

4/4: Where is the SSMIS L2B NRT data stream obtained? The SSMIS L2B NRT data are obtained from EUMETSAT via the EUMETCast distribution system, in BUFR format. This distribution by EUMETSAT is a redistribution of data from NOAA.

4/22: Units for "220" and "60"? The brightness in this image gallery is non-calibrated number between 0 and 255 and it does not have unit. This has been explained in the text.

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. Ok, it has been included in the discussion. The intention with the classification is to create a realistic spatial distribution of sea ice for input to the imaging simulator. We selected an image covering a large area with both 100% ice and open water and all concentrations in between to compare the actual ice concentration as given in the image with the simulated ice concentration using the imaging simulator and the spatial resolution of the microwave radiometer. The mismatch between these two is the smearing. We found that the smearing was relatively uniform in between the 0% and 100%. Logically the smearing is zero at the tie-points.

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

been changed.

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 reviewed journal articles that could be cited: Dedrick et al., Canadian J. Remote Sensing, 2001; Partington et al., JGR, 2003. The reference has been included. We have reprocessed the ice chart intercomparison with the NSIDC and NIC datasets and extended the comparison to cover 1978-2015. The datasets that we have used have been specified in the text.

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. Ok, we have done as suggested.

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. Ok, we have deleted the redundant sentences.

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? OK, we have redone the intercomparison using the data that are publicly available and documented on NSIDC and NIC. This has also resulted in a longer validation until 2015. We have included references to these documents data and procedures.

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. Ok, we have merged the sections and deleted the redundant sentences according to your suggestions.

9/28 – 10/1-3: repeats 9/21-22. Ok, 9/21-22 has been deleted.

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"). Ok, the "geo-location error section" is now part of the "first component . . ."

10/26: Again, isn't smearing (2.9) part of the representativeness error (2.8)? Yes, 2.9 has been made a subsection.

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. The monthly varying ocean masks are based on the NSIDC monthly maximum ice extent climatology, available at http://nsidc.org/data/smmr_ssmi_ancillary/ocean_masks.html. The extent in these ocean masks have been extended with 100 km (in all directions). For the extension of the data record from 15.10.2009 the extension distance was increased from 100 km to 300 km for the Southern Hemisphere, as the ice extent has increased in some areas in the recent years.

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. Ok, we have revised this section.

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. Ok, the "only" has been deleted.

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...." Ok, it has been changed according to your suggestion.

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). Ok, we have restructured this section with clear explanations of the procedures.

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

15/26: repeat of 15/16 Ok, 15/26 is deleted.

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. We haven't done an in depth analysis of this. The open water bias does seem to have a decreasing trend. We think that this might be due to better quality NWP data from 1978 to 2015. It has been discussed in the text.

16/8-9: Ocean cells have above 0 concentrations even with the atmospheric corrections? What about the smearing error – couldn't that contribute by smearing out the ice into open water areas near the ice edge? The atmospheric correction is reducing the variability over open water but it does not totally remove it, and we are not correcting for cloud liquid water. Some of this residual bias can be regional leaving above 0% ice concentrations over open water in some places. This is not optimal, however, we wanted to avoid the use of weather filters with fixed thresholds. Weather filters with fixed thresholds are not consistent with our methodology (using dynamical tie-points and regional error reduction) and they may introduce artificial trends.

The smearing error is an issue near the ice edge and this is also evident in the uncertainty estimate. We have extended the discussion on these issues.

16/30: National Ice Center should be capitalized. Ok.

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. Ok, it is deleted.

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. You are right. The sentence has been deleted and the possible geo-physical explanation given.

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

18/2: Who are the "two other producers"? Is this referring to other ice charting centers? Yes, it has been specified now (Norway and Greenland).

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

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? The reason for using 30% in this study is because it gave less noise in the example applications. When not using weather filters there is still a residual noise over open water.

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? An explanation is included in the text. The variability that the 5 and 95 % are showing is the daily variability within each month of the record.

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

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? Right, this is now clarified in the text.

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?

The significance levels on the short period indicates that the trend results are in general not significant and it is likely, as you say, that one or two unusual years dominate. However, for the long record most of the trends are significant (except perhaps along the ice edge) and the Bellinghausen Sea and East Antarctica is having more open water days and regions in the Ross and Weddell Seas are having less open water days.

The figures showing the short period (2004-2014) have been removed for the reasons that you mention.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-34, 2016.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2016-34/tc-2016-34-AC2-supplement.pdf

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-34, 2016.