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The Cryosphere Discussions

Interactive comment on "A scatterometer record of sea ice extents and backscatter: 1992–2016" *by* Maria Belmonte Rivas et al.

Anonymous Referee #2

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General comments:

The manuscript "A scatterometer record of sea ice extents and backscatter: 1992–2016" by Maria Belmonte Rivas et al. (tc-2018-68) introduces a data record of sea ice extent and backscatter merging different space-borne scatterometers (ERS, QuikSCAT, and ASCAT). In addition to presenting the methodology and resulting data record, the authors use the new almost 25 years long data to describe recent changes of Arctic sea ice observed after the September 2007 minimum.

The contribution is generally of fair quality, and the text is understandable. The paper could be improved by giving some more details on the methodology, and discuss uncertainty of the retrievals.

A major concern are the shortcuts taken when comparing the capabilities of scatterometers (this study) versus (passive microwave) radiometers (e.g. NSIDC and OSISAF SIC data records), especially when it comes to summer melt. The claims of the authors on that specific topic are too often not supported by facts.

I encourage the authors to revise the way their findings are presented (not the methodology they apply to compute the data records) and to consider the comments below as an incentive to enhance the quality of their contribution. They should strive at being more balanced, and avoid shortcomings in the presentation of their results.

As a general comment : all the plots (especially the maps) are too small.

Specific comments:

Abstract: "providing a means to correct for summer melt ponding errors". This is an overstatement. Unless you include in your study an investigation of the melt-pond season (May-June-July), for example using independent melt-pond fraction information (as done in Kern et al. 2017), the statement is not supported. You should be cautious with any statements about the alleged superiority of scatterometers to measure sea ice under melt-ponding conditions : the radar backscatter signal will be very much influenced by the surface water, and by the melting snow. In these conditions, it is hard to believe that scatterometers will have the accuracy to partition the 0%-30% reduced ice concentration (as measured by passive microwave) into some percents of pure "open water" contribution and some percents of pure "melt water" contribution. Such a partition however is what it would take to "provide a means to correct for summer melt ponding errors".

page 1 line 29: "instances of missing thin ice" . . . from where is it missing? from the passive microwave records?

page 2 line 5: when comparing sea ice estimates to operational ice charts, especially during summer, one should always question the accuracy of the charts.

page 2 line 14 : “0.1 dB via buoy collocation” (do you mean “via collocation of wind retrievals at ocean buoy locations”?)

page 2 line 16 and 17: “which are known to cause discontinuities...”. please consider the following wording: “which -if not done properly- can affect long-term trends in sea ice concentration”. It is fortunate that the inter-calibration of brightness temperatures is generally well taken care of by expert teams prior to computing data records of sea ice concentration. Titchner and Rayner (2014) discuss the stitching of SIC from different sources (passive microwave, navigational ice charts, etc..) and not the impact of non-optimal inter-calibration of passive microwave sensors can have on SIC trends.

page 2 offers a good occasion to re-state that passive microwave records are primarily aiming at mapping sea ice concentration, and sea ice extent is not a goal as such. sea ice extent is only a downstream indicator.

page 2 line 21: “It is known that...”. Then please rather add a good citation.

page 3 line 4: when referring to the ice age data from NSIDC, a more recent citation is 10.1109/JSTARS.2010.2048305.

page 3, line 20: which Metop platform(s) are used for ASCAT? please specify in the text.

page 3 line 25: It would be useful to mention here the zenith angles of the different instrument (for example include here the first sentence of section 2.4).

page 3 line 25 : is it worth briefly mentioning NSCAT and why it was not selected in your data record?

page 5, line 5: “stable wintertime backscatter levels” I understand this as your ice GMF is static, and not dynamically updated (e.g. with months). If I misunderstood, please consider making the sentence more explicit.

page 6, line 13: please use one sentence to explain the difference in degrees of free-

dom.

page 6, eq (7) : this is presented as the formula to compute daily estimates of the probabilities, thus supposedly gathering several passess of a given instrument. Yet, equation 7 does not show any indices. Could you make the indices appear in eq 7? Are the individual probabilities multiplied or averaged together?

page 6: it would be very interesting for the users to extend section 2.3 with some material to describe what would be your approach to providing quantitative uncertainties. A key discussion point might be how to treat that the PDFs of individual observations might be correlated with each others. To the least, uncertainties in the retrievals and how to quantify them should be discussed at the end of the manuscript.

page 6: two informations are missing from section 2: 1) how do you define your SIE (from daily maps of sea ice probability... what is the threshold on probability?) and 2) describe in what sense “these algorithms have been tuned to match the passive microwave sea ice extents during the fall and winter months” (ref your page 2, line 11).

page 8 lines 9, 14, 15, 18: the use of “biases” is problematic as it implies you consider one source (the scatterometer?) is correct and the other (the passive microwave) is at an offset. Replace with “differences” at all 4 occasions.

page 8, lines 7-8 “the agreement... is of comparable high quality during the freezing season”... is it surprising? “these algorithms have been tuned to match the passive microwave sea ice extents during the fall and winter months” (ref your page 2, line 11).

Figure 7: are the colorbars for SIC correct? They would indicate that dark blue is for everything below 20%? It would be interesting to show contour lines of the sea ice extent from NSIDC-0051, OSI-430, and SCATT (e.g. on top of the NIC chart), as this is impossible to observe from the colored maps of SICs.

Concerning Figure 7 : being an operational product meant for tactical navigation in the ice, the NIC ice charts are not a reference for accurate SIC monitoring. It uses

active microwave data (SAR) that might suffer from exactly the same noise sources than the scatterometers, namely that scattered, faceted pieces of ice will seem brighter (higher backscatter) than if the corresponding area of ocean was covered by contiguous ice. Always question the accuracy of NIC charts (or other navigational ice charts), especially during summer.

Page 10: "surface wetness and melt ponding ... during spring and summer [Comiso,...]" 1) consider also citing Kern et al. (2017) https://doi.org/10.5194/tc-10-2217-2016 as a more recent and quantitative assessment of the impact of melt-ponds on the passive microwave retrievals of SIC.

Concerning Figure 6: it shows a mismatch between PMR and SCATT SIE for both hemispheres and the whole of spring+summer seasons, all the way until the SIE minimum. The sentences following Figure 6 (and Figure 7) point at "surface wetness and melt ponding" as a reason for the mismatch. This is not very intuitive, since:

1) the PMR SIE is defined as the area where SIC is larger than 15%. Although the impact of melt-ponding can be up to 20%-30% (Kern et al. 2017) at the maximum of the season, most of the melt-pond covered area will still show SIC>15%, thus only marginally influencing the PMR SIE.

2) Regardless of item 1), melt-ponding (on sea ice) is happening mostly in (late)May, June, and July, (early August) before they drain through the ice. There are no melt-ponds on top of sea ice in mid September (Figure 7). In addition, melt-ponds on top of sea ice are mostly (if not only) observed in the Arctic. Melt-ponds can thus not explain the SH mismatch you document.

However:

1) it is noted that the SCATT SIE has a finer spatial resolution than the SSM/I and SSMIS PMR SIE (this is obvious on Figure 7) due to the size of the FoVs of SSMIS at 19 and 37 GHz. Spatial resolution has been documented to have a definite influence on

the SIE metric (Notz, 2014, https://www.the-cryosphere.net/8/229/2014/). Can spatial resolution have an influence on the SCATT SIE?

2) In the marginal ice zone, geometric scattering effects by disjoint, faceted ice floes will induce higher backscatter, and thus artificially enhanced radar "brightness" than if the same quantity of sea ice was present in a planar, contiguous way. The PMR signal is insensitive to these geometric effects. Could this geometric effect explain the difference in SIE you observe on Figure 6?

3) Finally, if indeed your sea ice GMF is static and tuned for winter conditions (see page 5, line 5), then you should discuss if the spring+summer sea ice GMF is similar (than the winter one) so that to ensure that seasonality of the sea ice GMF does not artificially contribute to seasonality in SCATT SIE.

All in all, and as noted in the introduction to this review : the readability and impact of this paper would be greatly improved if 1) you described the observed differences between PMR and SCATT SIE without too hastily attributing them to deficiencies of the PMR SIEs (specifically melt-ponding), 2) you investigated (or pointed to earlier investigations) how other factors might explain (better?) the differences.

Figure 11: a legend/colorbar is missing. In addition the maps are too small (this applies to all figures in the draft manuscript). Figure 11 shows large discrepancies between the left and middle panels outside the central arctic ocean (e.g. second year ice in Bering Sea in 2008 for ASCAT but not QSCAT). Please either zoom your maps to the Arctic Basin (this is the area you discuss anyway, or comment the large discrepancies (how they can be mitigated).

Page 15, line 18: as noted later in the text, the OSI-SAF product OSI-403 does not hold a MY ice concentration, but a MY/FY classification.

Figure 13: Several remarks:

1) the panels are too small.

2) what date is this from (inside March 2016)? Or is this a monthly average for all panels? If a monthly average you should probably describe how the average is performed, given the variety of input variables (MYI conc, MYI classification, max SIA, mean SIA,...). It would make more sense to have a specific date.

3) consider adding coast lines.

4) legend : the OSI-403 is not a MYI concentration product but a sea ice classification.

5) please add a “first year ice” color (or a sea ice edge line) on panels b) to f).

6) please check your plot c) and f) (from Korosov et al. 2017 data). if this is plotted from variable “sia” (the mean sea ice age), one has to look at 1<sia<2 for the second year ice (as soon as sia is > 1, then it has 2nd year ice contribution). The result will look much more similar to NSIDC SIA and the other maps, especially in the Beaufort Sea.

7) the description of the location of the features is difficult to follow in the text, would it be an idea to write letters on the map (A, B, C, ...) and refer to them?

8) all estimates (but yours in a) show a thin tongue of older ice extending across the Arctic Ocean towards the New Siberian Islands. It can even be noticed somewhat in the Cryosat-2 thickness. It is visible on the shades of “normalized backscatter” in your panel a). Given the variety of methods used for all other panels, are you confident that your new estimate in a) is correct and that there is no such tongue of older ice? Discuss.

Page 17, line 15 : “as verified by comparison to NIC sea ice charts”. NIC sea ice charts are not the truth. Please use a different wording than "verify"..

Page 17, line 18 : “typically underestimating the summer sea ice concentration and summer sea ice extent by up to 30%”. Which one? SIC or SIE? As discussed above an underestimation of high SICs, even by 30% will have limited impact on the SIE (defined as SIC>15%). Rewrite.

page 17, line 21: “providing a solid basis to monitor the occurrence of sea ice concentration errors due to melt ponding”. NO. See notes from the introduction. Rewrite.

page 17, line 29, 30: this sentence is an exact copy-paste from previous section. Avoid word-by-word repeats, please.

page 18, line 15 : to use Sea Ice Type (or Age) as a proxy for Sea Ice Thickness is not a new idea. It would be good to cite earlier attempts e.g. Tschudi et al. 2016, http://www.mdpi.com/2072-4292/8/6/457.

Technical corrections:

page 1 line 14: “record sea ice loss” : consider another term than “record” as it is used elsewhere in the paper (including the abstract) as “data record”.

page 1 line 19: “1978” (with the start of SMMR ni Oct 1978). There are passive microwave instruments before (e.g. ESMR), but the routine monitoring indeed starts in 1978 with SMMR.

page 3 line 22: “mission transition periods”.. consider “mission overlap periods”

page 3, line 13; “measurements about extended” . . . do you mean “around”?

Figure 4: please add a second legend with solid line for NH and dashed line for SH.

Figure 5: please add text in the plot area for NH (top) and SH (bottom).

page 7, line 15 : you are probably referring to OSI-430, that extends OSI-409a (http://osisaf.met.no/p/ice/index.html#conc-reproc). Same in caption to Figure 7.

page 8, line 9 : “Quikscat-to-ERS”... you probably mean “ERS-Quikscat”?

Figure 7: you seem to have a dip in QuikSCAT SIE in antarctic curve for 2000 (around day 260).

page 10, line 24: “arrival of summer signatures” consider “appearance of summer signatures”?

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