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Interactive comment on "Satellite Passive Microwave Sea-Ice Concentration Data Set Intercomparison: Closed Ice and Ship-Based Observations" *by* Stefan Kern et al.

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Review of "Satellite Passive Microwave Sea-Ice Concentration Data Set Intercomparison: Closed Ice and Ship-Based Observations" by Stefan Kern et al.

by François Massonnet

Summary ———

Sea ice concentration (SIC) datasets are essential for a number of polar applications. Evaluating their accuracy and precision is therefore necessary to gain confidence in the value of those datasets. This study performs an assessment of ten popular products

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obtained from satellite microwave radiometry, against independent data. The goal is to inform users of those datasets about the accuracy and precision of each dataset. A secondary goal of the study is to question current practices, like the truncation of SIC at 100% or the dangers of using threshold-dependent diagnostics (like sea ice extent, SIE) when weather filters are applied.

Overall I found this study interesting and, being a user of several of the products presented here, I am now more alert about pitfalls that such products can have. The main strength of the manuscript is that it provides a systematic and thorough assessment of the products, with many maps/histograms/time series that users can refer to when they will be using the data themselves. I also appreciate the work done in the Appendix to document the various algorithms. Having this information in one single paper is really a plus for the scientific community and anyone susceptible to work with this data one day. The main weakness of the manuscript is that sections 3, 4, 5 are overly long and descriptive without interpretation of the results (which comes in section 6). To be honest, paragraphs like the one at line 613-621 are difficult to follow. So many numbers, statistics and product names are given that it is just not possible to process the information when reading. So, I feel that the text could improve in clarity if the authors chose to focus the description on a few key points rather than trying to exhaustively describe the figures each time.

So, the paper is worth publishing in a journal like the Cryosphere but I think it would deserve a haircut in the middle sections because this is where the authors will lose most readers, unfortunately. Drowning readers with too detailed information in the text has the danger that the most interesting bits (like the issues of truncation at 100%) go unnoticed, so I think it's worth the effort.

On top of that, I do have a few general, minor and editorial comments that I hope the authors can address.

General comments and questions ————————————-

- The manuscript revolves around key concepts of metrology like accuracy, precision, uncertainty, error. To my knowledge, the ocean community has tackled the issue of defining the meaning of each term, while the sea ice community has not yet done so. Several authors of the manuscript organized a fruitful workshop in October 2017 where those questions were addressed. The present paper represents a timely opportunity to introduce those concepts to the cryosphere readership, so I wonder if one additional section or paragraph could not be devoted to explaining those basic concepts. The work needed is modest but the impact can be high, especially in terms of citations of this paper later on.

- Having used the ASPeCt data myself, I know that this dataset, while very valuable, has to be taken with extreme caution. The reason is that (1) individual measurements are prone to large errors because they are estimated visually, (2) the data is not necessarily representative since the ships preferentially navigate in thin ice areas, so retrievals might be biased thin (for sea ice thickness) with possible adverse consequences on SIC as well (I suppose, biased low) (3) co-locating the data is challenging. Points (1) and (3) are covered in the manuscript (nice discussion at the head of Sect. 5, by the way), but the point (2) would deserve one or two lines of additional comments.

- I like the idea of creating groups of products that share similarities, that's a novelty as far as I know. I was not entirely clear though if those groups were built based on March Arctic SIC (as introduced at line 345) or following a more global criterion? That is, are the groups defined based on expert a priori knowledge, or are they based on the SIC fields they produce? According to my understanding, the former approach was chosen. But then, are the groups stable when the hemisphere changes, and when the season changes? It would be useful to know for the users if products that are close together in winter in the Arctic are also close together for summer and in Antarctica, for example. If not, a few explanations would be welcome.

- In the scatterplot analyses (Figs. 15 and 17), a surprising result is that the evaluation is strongly dependent on whether one bins the satellite data to the ASPeCt categories

(red colors) or one bins the ASPeCt data to the satellite categories (blue). I could not find in the text a satisfactory explanation of why this is so, could the authors elaborate? Can the better agreement in the case of red colors be used as justification to favor one evaluation method over another?

Minor comments (starting with a line number) ——————————————————

81 Two studies are mentioned in which SIC was evaluated using independent data. It would be good to end that paragraph with a sentence stating the main limitations of those studies (Andersen 2007 is quite old I guess, Beitsch 2015 is only focusing on the Antarctic?) and why the present study is necessary.

88 The sentence "We focus on differences..." sounds like this paper will focus on winter conditions, and the summer sea ice concentrations will be analyzed in a subsequent paper. But the present manuscript covers in fact summer as well. Why would a second paper be needed then? Is it more the type of independent data for evaluation that will change?

104 What is meant by "continuation"? I understand that the dataset production must be going on, but I am unsure.

146 I'm not entirely clear regarding the diagnostic used here (and in Fig. 1). From what I understand, only SIC in the range]0 %, 30%] is retained. Then, for each day of the month, one identifies what the 5% percentile of that SIC spatial distribution is (not all grid cells participate due to the restriction to the quoted interval). Finally, those 5% percentiles are averaged in time. Is that correct? Do not hesitate to spend one or two extra lines to avoid ambiguities in how the diagnostic is computed.

147 Along the same lines, I'm not sure to understand why the diagnostic would not be impacted by secular sea ice trends and why stability is expected. The SIC distribution has been profoundly changing over the past decades (mean and higher-order moments), so why would percentiles not be impacted as well? In fact, it would be quite

remarkable that a sea ice diagnostic, especially an Arctic one, would not exhibit trends over several decades. Clarifications would be welcome here.

151 As far as I know, the computation of SIA does not interfere with the choice of the 15% SIC threshold, so that statement only applies to SIE?

178 The title of section 2.1.4 is "Distribution around 0% and 100%" but the section actually deals with near 100% concentration. This is a bit strange or I missed something.

180 "sea-ice concentration on either side of 100%" is a sentence that could puzzle a few readers. I think I understand what it means, i.e. that the algorithms are designed in such a way that, in presence of noise, their output can yield above-100% values for sea ice concentration, and that some products apply truncation. But it would be good to write this down in one or two sentences. This is somewhat explained in the conclusion (line 943) but should appear earlier in the manuscript.

205 It would be good to know by how much the "near 100%" SIC values of the RRDP2 dataset deviate from 100% in the Areas of Interest. Are we talking about a maximum 1% deviation, 0.1%? Is this information available in the cited papers?

288 What is the origin of the 98% value used for filling the pole hole? Why this choice?

318 I find interesting the result that the spread for SIE is less than for SIA. This finding would warrant more discussion because it highlights that SIE, despite being certainly a less physical diagnostic than SIA, offers advantages as soon as one is not so much interested in errors above 15% SIC. Linking this finding to earlier papers by one of the co-authors (Notz et al., https://www.the-cryosphere.net/8/229/2014/tc-8-229-2014.html) would be a plus. In particular, what are the implications of Fig. 6b versus 6c when it comes to model evaluation? If I would like to evaluate an atmosphere forced ocean-sea ice simulation and focus on the iconic year 1996 and in September, for example, do I have an interest in using SIE instead of SIA? Or is it risky to use SIE because it could hide error compensations? I noticed that this is briefly touched in the

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paragraph at line 677, but it would deserve more discussion. In the current state, Fig. 6 could leave the feeling that the use of SIA should be discarded because it is less precise than SIE.

326 The deviations in SIC are computed as the difference of each observation and the ensemble mean. Given the small sample size, wouldn't it better to compute the difference with the median instead of the mean? The risk is that if there is an outlier, it will drag the mean towards itself and differences could not be so large. By choosing the median, one increases the probability to detect outliers (I think).

344 I find interesting the idea of clustering the products in three groups. It would be useful to write the group name in each of the panels of Figs. 7, 9, 10, in order to visualize how this translates in terms of spatial maps.

428 In the discussion of the near-100% SIC (this section and Fig. 12), it would be useful to tell the reader what a perfect product would look like. I assume, such a product would have all its power concentrated in the upper bin of Fig. 12 (almost like NT2-AMSRE), correct? In essence, I'm asking if the authors could interpret the results of Fig. 12 in terms of performance. From the current discussion in section 4.1, it is unclear if a few products are superior to others for that diagnostic.

435 I'm not sure to agree with the sentence "The distributions for these products look like as if one has taken the distribution of, for instance, OSI-450 but deleted the fractions of bins...". First, I don't quite see that from Fig. 12a. Second, by construction, the cumulative fractions have to sum to 1.0, so that products with smaller ranges have to compensate by higher frequencies. So they cannot simply be a truncated version of the other cumulative fractions, right?

Editorial /style comments ————————————-

50 The meaning of "spatially and temporally resolved sea-ice concentration" is clear to me. Are there datasets that are not spatially or temporally resolved? Could you clarify,

please?

94 "It" grammatically refers to "input" while I think the authors mean "Section 2".

167 I wonder if "hockey-stick" is appropriate here. To my knowledge, this term was initially used for describing the shape of mean temperatures over the past millennium, with a slight decrease until 1800 followed by sharp increase later on. The shapes in Fig. 1 are arguably not following the same logics.

180 "the EUMETSAT-OSISAF – ESA-CCI products": could you name explicitly which products you refer to?

325 "Arctic sea-ice distribution" –> "Arctic sea-ice concentration distribution"

344 Fig. 11 is referenced but Fig. 10 and 9. have not been referenced yet.

369 "Antarctic sea-ice distribution" –> "Antarctic sea-ice concentration distribution"

429 I would change the sentence "As expected the cumulative fraction increases towards 100% for all products" into ""As expected the cumulative fraction increases towards 1.0 for all products". The cumulative fractions are expressed as units in the figure.

1286 Please add the units of frequencies in the middle column of Table 1.

1357 Figure 1 labels in several panels (all but a and g) are too small to read, at least when printed on paper.

1367 The meaning of the grey lines in Fig. 3 is not clear to me. The figure caption mentions black symbols and lines. I suppose the former is the vertical dashed lines at 100%. Is the former the grey lines then? Please clarify.

1377 The colormap used for labeling the years in Fig. 5 might not be colorblind-friendly. Consider changing it.

1397 The lower and upper limits of the color bars change depending on the diagnostic

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(SIE vs SIA) and the hemisphere. I think it would be better to use the same limits (-2 – 2 or -3 – 3 million km^2), this will then highlight the relative merits of using SIE or SIA, and also highlights that winter Antarctic spread is very large.

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