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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.

Anonymous Referee #2

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Summary:

This paper presents detailed comparison and analysis of several passive microwave sea ice concentrations. A general intercomparison between products is done evaluate biases between products and differences in trends and variability. Then the concentrations are evaluated by comparison with a validation product consisting of high-confidence 100% concentration observations, and with ship-based observations. The results showed varied performance by the different products, which depend on regional and seasonal conditions.

General Comment:

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Discussion paper

This is a very detailed manuscript and will serve as a solid reference for understanding differences in the different passive microwave products. The analyses are logical and thorough. The manuscript is generally well-written, although there are some areas of repetitiveness and verboseness that could be shortened. A main issue of the paper is that it is quite long. In some ways, it almost feels like two or even three papers – each part (the product intercomparison, the 100% validation, and the ship observation comparisons) could be a paper in its own right. However, I can see the value in having all the information available in one document. Thus, much of the length is justified by the content, but I do think some areas could be made more concise, and while there are already substantial supplementary appendices, perhaps more material could be shifted to there.

Another issue is that there are a lot of numbers thrown around – biases, RMS, R-squared values. These can be hard to read inline in the text and seem to be well-summarized in the figures and tables. Thus, I would advise considering focusing less on the numbers in the main text and more on the fundamental behaviors (e.g., lower biases vs. higher biases).

Overall, I would say minor revisions are required. The biggest effort may be in shifting/cutting some text, but I don't think this would be too difficult. Specific comments are below.

Specific Comments (by line number):

111-112: Important to note here that it is a polar stereographic grid and is not equal area (that's implied with the 70 latitude comment, but should be stated explicitly for clarity)

126-176: I know the weather filters are quite critical, but these seems to be algorithm info that might be best shifted to the Appendices with the other algorithm info. Maybe here simply note why the weather filters are important to the study and leave the details to an appendix.

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Discussion paper

233-237, 247-254: an area where things could be shortened. The first part talks about how the Arctic uses a different protocol than ASPeCt, then the part discusses that protocol, ASSIST. These could be combined and shortened so you have: Antarctic, ASPeCt; Arctic, ASSIST.

280-281: One area not clearly explained is how the monthly average concentrations were derived. Was it simply the average of the concentration for all days in the month? Was a 15% cutoff used?

290-310: this section seems either repetitive of early content (weather filters), or what comes later. Figures 6-11 are all introduced here, but aren't discussed until the following sections. That means the figures are referenced twice, which is wasting words.

344-352: Outside of 290-310, Figure 11 here is referenced in the text before Figures 8, 9, 10. Taking my suggestion for 290-310, then Figure 11 shouldn't be mentioned until after Figures 8-10. You could split Figure 11 into separate Arctic and Antarctic figures and reference the Arctic here. Or simply wait and add this discussion for both hemispheres at the end of Section 3.

354-362: One thing not really discussed in detail is the impact on spatial resolution on the results. At least some of the biases between products – especially for SIE – is due to different spatial resolution (not only gridded by the sensor footprint size), particularly between AMSRE and SSMI-based products. This should be discussed in some detail to put the results in context. As noted, for area, the effect is small because the concentrations tend to be low at the ice edge, but for SIE, even a 25 km resolution effect, summed around Antarctica can be 100,000s of sq km in SIE.

453-456: I guess a question here is whether the truncation is done intentionally. Setting the tiepoint to get a mode a little higher than 100% does, as the authors note, improve the statistics. It's admittedly for the wrong reason and can result in further problems and it does skew estimation of uncertainty. But I can also see the logic to set things a bit high to avoid underestimation at <100% concentration. I guess one question here

is: how much difference does it make? Does setting the mode for 100% at 101% or 102% make a significant difference, e.g., in terms of heat fluxes? This is discussed in another comment further below.

402-528: This section is quite long and somewhat difficult to read through and keep track of all of the numbers. This is a section that could perhaps be shortened by leaving numbers to figures and tables and focusing on the key takeaways.

606-608: But the authors note earlier that these are the concentration ranges where ship-based obs are least reliable. So, are the differences errors in satellite or in the ship obs?

694-697: As noted above, spatial resolution of the TB input plays a big role in the location of the ice edge, weather filter, and land-spillover,

660-882: Section 6.1 is really long and seems to largely repeat the results section before. I think much of this could be shortened or removed completely. I don't think figures should be referenced again – they go with the results. Lines 822-831 are a nice summary and this is what Section 6 should be aiming for, not repeating all the results. Section 6.2 is also good – could almost just do that and remove 6.1.

877: The summer to winter differences in ASI are likely largely due to the atmosphere, right? More moisture in the summer and a bigger effect on TBs. This should be noted here.

900-955: The conclusion also feels very repetitive and overly long. Focus not on just restating results, but providing a concise summary and drawing relevant conclusions.

936: They're empirical models, not "geophysical", right? At least in general.

Appendices: Strictly speaking, the algorithms could just be referenced to their primary source material. However, since these are appendices, and the summaries are nice to have accompanying the paper, they are okay. These could be shifted to supplementary material though, as opposed to appendices in the main manuscript.

Minor Comments (by line number):

60: The acronym for “SMMR” should be spelled out here, the first time it is used.

90, 92: “contribution” sounds odd to me. These are manuscripts, so perhaps just say “manuscript” or “paper” or “journal article”.

142: replace “roots” with “is based”

147: not sure if this is a typo or intentional “]0.0% to 30.0%]” – the backward bracket is not something I’m familiar with. I don’t see the need for brackets at all. This happens elsewhere in the manuscript as well.

158-159: incomplete sentence

190: replace “comply with” with “be consistent with”

289-290: this sentence isn’t clear to me.

355: “1980s and 1990s” – no need for “ties”

372: “< -20%” – here you mean, e.g., -30%, -40%, etc., right? As written it’s correct, but it can be ambiguous because “<” can imply lower in magnitude. So maybe be more explicit here to be clear, e.g., “differences in negative biases even greater than 20% in magnitude”.

575: “satellite-based”

591-593: the phrasing is awkward in this sentence – hard to follow

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