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Interactive comment on "Satellite passive microwave measurements of sea ice concentration: an optimal algorithm and challenges" *by* N. Ivanova et al.

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Summary

The manuscript presents an optimal approach to derive sea ice concentration from passive microwave imager. Thirty candidate algorithms were evaluated, noting stability, sensitivity to atmospheric conditions, and performance in different ice conditions such as surface melt and thin ice. The optimal algorithm uses a combination of two algorithms with atmospheric corrections from NWP data and dynamic tiepoints.

General Comment

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Interactive Discussion

Discussion Paper

This is a very nice analysis that builds upon previous work and presents a very thorough analysis of a wide variety of algorithms suggests and optimal strategy for best overall performance. There are few fairly minor issues to address, noted below, but after those are addressed, I recommend publication after minor revisions.

Specific Comments (by page and line number)

1271, 15: I know there may be a length limitation in the Abstract, but if possible you should at least briefly describe the optimal approach. As it stands now, it says an optimal approach has been suggested, but no information on what that approach may be. Just another sentence saying that it is based on the combination of two algorithms, atmospheric correction, and dynamic tiepoints.

1274, 13: thin ice concentration estimation significant for ice volume? How big of an effect is this? Because the ice is thin, it seems like it would have a minimal effect on volume. Even at 1 million sq km of 30 cm ice being "missed", that's only 300 cubic km in volume. I guess, especially with low volumes that are seen now, that could be up to 5%, though I think generally it would be more like 1%. I doubt ice volume estimates are accurate to even close to 1%. And that underestimation is in some sense temporary because the ice (during winter growth) will fairly quickly thicken to >30% and not be underestimated (or at least underestimated as much). I guess the main thing here is not that it's irrelevant but the other effect – on air-sea heat (and moisture) exchange is much more important than the volume. So perhaps just separate out those two, e.g, "significant effect on air-sea exchange" and "also effects ice volume estimates".

1277, 1: The RRDP is introduced here without any explanation, so it's a bit confusing as to what the authors are referring. The RRDP is later explained, page 1284, lines 1-9, but the reader is left in a bit of limbo for 7 pages. I would recommend explaining RRDP as it is first mentioned.

1278, 29 – 1279, 4: This text is really simply describing the contents of the figure, so it would be best left to be in the caption and not in the main text of the manuscript.

1286, 5: "ECICE algorithm was adjusted...in this study". Why was it adjusted? How was it adjusted? More info is needed here.

1286, 20 – 1287, 21: I'm a little confused on the melt pond analysis. If I understand correctly, the authors are comparing the retrieved PM concentrations with the concentration of non-ponded ice retrieved from MODIS and finding that PM is overestimating concentration. In this framework, I can see why the PM overestimates, and I don't think that's not necessarily a bad thing. The authors assume that PM see melt ponds as open water, and to some degree that makes sense because generally the penetration depth of PM is small. However, I'm not convinced that a melt pond is the same as open ocean water in the PM signature. Melt ponds are quite different than ocean water (e.g., in leads) – ponds are fresh water on top of ice cover. So I would expect that there could be a different signature. It could be that the algorithm are "tuned" through tie-point selection to see melt ponds as ice-covered.

Fundamentally, what I'm saying is that the authors seem to be suggesting that PM algorithms should detect ponds as open water and that concentration retrievals should reflect only non-ponded ice – i.e., if there is 10% open water and 40% pond coverage, the authors seem to suggest that an accurate concentration retrieval would be 50%.

I'm not sure that this is optimal. Ponded ice is still ice, so I would say that 10% open water and 40% would be best retrieved as 90% ice concentration. Now, granted, 90% ice with 40% pond coverage is very different than 90% ice with no ponds. However, 90% ice with 40% ponds is very different than 50% ice and 50% open water – whether it be for navigational support (not that it's advisable to use PM for navigation), calculating radiative fluxes, input into models, etc.

I suppose this is somewhat of a value judgment, but to me a better approach is to try to get the concentration as accurate as possible and let melt ponds be calculated separately (e.g., with the MODIS product).

The authors' approach is no less legitimate I suppose, but I think some further discussion is warranted, either here and/or in the discussion, pg. 1293, line 16 through pg. 1294, line 17, to discuss the ramifications of how ponds are addressed (or are attempted to be addressed) in the PM algorithms.

1291, 12: I see he tie-point variation is 8-10 K in Figure 8 and that that is 8-10% of the average tie point, but this is from the Bristol "y-component", right? But many algorithms use simple ice tie points, which are 200-250 K. Would the 8-10 K apply there, in which case it would be more like 3-5%, or would the variation be more than 8-10 K? For the open water, which is a simple surface type tie point (Fig. 8 b and d), the variation looks to be only 3-4 K. I would expect the OW tie point to have less variation than ice tie points, but I wonder if the 8-10% variation from the Bristol is a function of the combined y-component tie point approach or if it would apply to simple ice tie points – i.e., is the variation for those 8-10% as well, meaning 15-20K?

1291, 19: Table B1 is quite interesting and points out an important issue to consider – sea ice trends due not to changes in sea ice but due to sensor drift, intercalibration, and trends in atmospheric variables that effect the sea ice retrieval. However, the numbers presented in the table do not give a real good sense of how big of an effect this is. In other words, how different is the sea ice trend than reported due to these effects. I don't suggest the authors actually try to explicitly calculate this, but it's hard to get a sense of what general (e.g., order of magnitude) effect because the trends vary (even in sign) between sensors and the OW and ice tie-points also vary differently. To put it succinctly, if the current data say the Antarctic September sea ice trend is $\sim$+1% per decade, would these tie point effects potentially suggest that the trend is instead $\sim$-1% per decade? I suspect not, but it would be useful to have some sense of what these effects are on the overall trend estimates.

1298, 5: something seems to be missing here – "…temperature is the only one." The only one what? The only parameter that Bristol is sensitive to?

1298, 24-28: The authors make the important point that the Near 90 GHz are subject to greater errors due to the atmosphere, particularly near the ice edge and over open water. However, they do have a distinct advantage (at least the algorithms that use only the near 90 GHz channels) in that the higher frequency channels have much smaller sensor footprints, higher resolution – roughly double the spatial resolution. This may or may not offset the atmospheric issues, but I think it is a salient point. While the time series for such products is not as long, the 1991-present timespan is potentially value for climate studies.

Figure 4: Both figures on the bottom row are labeled “Near90”. Should one of these be “NASA Team”?

Figure 4: The bias correction mentioned in the caption is not discussed in the manuscript text. What is this and why is this done? This should be better explained within the main text.

Minor Comments:

1278, 19: remove “got”

1279, 24: suggest “slope of one” instead of “slope of unit”

1281, 16: “substitution” instead of “substitute”

1281, 23: change to “. . .SIC values, though this does not apply. . .”

1288, 19: “. . ., see the introduction. . .” to “. . .; see the introduction. . .”

1288, 28: remove “real”

1290, 26: “An example of the ice tie-point. . .”

1291, 17: suggest “unrealistic” or “artificial” instead of “undesirable”. Also either “an artificial trend” or “artificial trends”

1292, 12: suggest “significant” or “substantial” or “large” instead of “severe”

1292, 18: “algorithm for a climate dataset” or “algorithm for climate datasets”

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1293, 6: “Similar” instead of “Similarly”

1294, 7: “. . .this effect: the OSISAF algorithm. . .”

1295, 2: suggest “limitation” instead of “drawback”

1297, 17: “all 10 algorithms. . .”

Interactive comment on The Cryosphere Discuss., 9, 1269, 2015.

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Interactive Comment