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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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The authors would like to thank Dr. W. Meier for the valuable comments that helped improving quality of the paper.

We will address the comments point-by-point (the answers are marked by A):

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.

A: adjusted accordingly.

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".

A: the authors agree with this remark and changed the text accordingly.

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.

A: The RRDP is now introduced in the last paragraph of the introduction.

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.

A: adjusted accordingly.

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

A: The wording was not clear in our text. The ECICE was originally developed for the Northern Hemisphere and we used this original version of the algorithm for both hemispheres. ECICE can be adjusted to the Southern Ocean by introducing a new set of probability distributions of the input parameters for each one of the intended ice types. This was not done in this research. This is clarified in the text now.

1286, 20 – 1287, 21 (1 of 7): 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.

A: Yes, we compare sea ice concentration from the PM algorithms with the ice surface fraction (free from melt ponds) as obtained from MODIS, and find that they are highly correlated. We also find that for these areas (ice between melt ponds and open water = concentration of the non-ponded ice) the sea ice concentration is overestimated by the PM algorithms. This contradicts what one would expect from theory because it seems as if PM algorithms retrieve sea ice where they should see open water according to theory because of the limited penetration depth of microwaves into liquid water. One potential explanation for this could be the effect of wetness of the surface on the Tbs causing thus higher SIC values.

1286, 20 – 1287, 21 (2 of 7): 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.

A: We agree that generally salinity should affect dielectric properties of a medium. However, for such high frequencies as used in the algorithms (19 GHz and higher) and in cold waters the salinity was found to play a less significant role (Meissner and Wentz, 2012; Ulaby et al., 1986). One may still argue that the observed signature of open water differs from that of summer melt (one might need a more specific definition

of summer melt though), first year ice, flooded multi-year ice, frozen melt ponds, crust, dry multi-year ice and open water as reported by Eppler et al. 1992. However, in application to satellite passive microwave measurements, this is rather uncertain. The footprint size in this case is so large (70*45 km for the 19.3 GHz channel on SSM/I) that an unresolvable mixture of surfaces might be present in it. In addition, footprint mismatch uncertainty is common for all the algorithms using more than one frequency, and we believe the difference in signature between melt ponds on ice floes and open water between ice floes will be within this uncertainty.

1286, 20 – 1287, 21 (3 of 7): It could be that the algorithm are "tuned" through tie-point selection to see melt ponds as ice-covered.

A: The overestimation by the algorithms we saw was presumably corresponding to the areas between melt-ponds, so in this case they (correctly) interpreted melt-ponds as open water with the set of tie-points used. However, the difference in dielectric properties of the sea ice between winter and summer seems to trigger overestimation of the sea ice concentration.

1286, 20 – 1287, 21 (4 of 7): 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%.

A: Yes, this is our conclusion, which is applicable to the sea ice algorithms based purely on satellite passive microwave observations from the existing (or formerly existing) instruments.

1286, 20 – 1287, 21 (5 of 7): 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.

A: We agree with this point. For many applications ponded ice is preferred to be identified as ice rather than water. However, we believe the algorithms considered are incapable of doing it. Our main points here are: a) satellite microwave radiometry is incapable to estimate SIC correctly if a certain fraction of the sea ice is submerged under water and b) it might be more straightforward to stay with what the sensor actually can do, and this is to estimate ice surface fraction. The latter will be similar to the well known SIC during most of the year - except in the melting season, when it will be a more accurate and more transparent estimation of the actual ice surface fraction. Why do known algorithms using satellite microwave radiometry retrieve close to 100% ice concentration in an area with only 70% ice surface fraction? This is not transparent and not easy to understand and can only be because the radiometric signature of the ice between the melt ponds has changed such that the plus in the open water at the surface does not count anymore that much. It can be assumed that this change in radiometric signature changes for different algorithms, which is why we have different scatter plots in Figure 4. As we are aiming for a climate data record we rather would like to provide the information the sensor can actually retrieve. Infrared temperature based retrievals of the sea surface temperature do also not aim to provide an estimate of the water temperature at 20 m depth. Moreover, infrared temperature based retrievals of the sea surface temperature have data gaps where there are clouds which cannot be penetrated by the infrared signal of the surface. These gaps need to be interpolated or simply stay as gaps. Here, with SIC we have the same setting: the microwave signal of the sea ice underneath the melt pond does not reach to the sensor. We have a data gap.

1286, 20 – 1287, 21 (6 of 7): 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).

A: We support this opinion. Data fusion might be necessary to retrieve more accurate sea ice concentration estimates in summer. In this work we have not approached this challenge as our purpose was to explore methods suitable for a consistent climate dataset, which would provide daily maps covering whole Arctic and Antarctic and cover longer time periods, which would be hard to achieve with MODIS due to the cloudiness, darkness and the length of available time series of the input data (launched in 2002). If the reviewers' approach would be to try to get the concentration as accurate as possible then we are on the right track because this is exactly what we are aiming for: to get an estimate of the ice surface fraction year-round with best accuracy and in a most transparent way within the physical limitations given by the sensors' viewing techniques.

1286, 20 – 1287, 21 (7 of 7): 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.

A: In addition to providing our opinions here (please see above) we will extend the discussion section in the paper to cover these 7 points.

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?

A: Yes, we show Bristol tie-points for ice because in the hybrid algorithm it is used for high SICs. The value of 8-10% variation is also valid for simple tie-points. Fig. 1 here shows Tb19V and Tb37V (ice tie-points) from Bootstrap F algorithm, where the variation is about 20-30K. In the updated version of the paper we will substitute the Bristol tie-points for ice by the ones from Bootstrap F because we found these to be easier to interpret as they are Tbs in K (while Bristol is using rotated axes, which are harder to relate to). Even though Bristol is used for consolidated ice, we still can use Bootstrap F example here to make our point about the dynamic tie-points.

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 _+1% per decade, would these tie point effects potentially suggest that the trend is instead _-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.

A: The authors agree with this point, the table raises more questions than it answers. Since we at the moment, indeed, cannot provide an estimate of significance of the effect, we choose to remove this table.

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?

A: Rephrased: "Over ice the chosen Bristol algorithm is sensitive to the snow and ice temperature profile as well as to ice emissivity variations. Surface temperature is quantified in most NWP models, which means that there is a potential for correction".

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.

A: The finer spatial resolution achieved by the higher frequency channels does not offset the weather-induced SIC biases over open water and near the ice edge. Model data used in the RTM to correct for the influence of surface wind speed, water vapor and air temperature have a coarser spatial resolution and hence will cause artifacts in the RTM-based correction of the input brightness temperatures. The remaining weather effects we cannot correct for (cloud liquid water and precipitation) will become even worse and more difficult to correct for because the model is even less capable to provide the information for this parameters at the same spatial scale as would be required and in addition the finer grid resolution increases the amplitude of the impact of e.g. cloud liquid water because gradients in these parameters are captured "better" and are less smeared. This will be mentioned in the text (the Discussion section).

Figure 4: Both figures on the bottom row are labeled "Near90". Should one of these be "NASA Team"? A: Yes, the bottom right panel should be "NASA Team", the misprint is corrected.

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.

A: From an inter-comparison between Envisat ASAR wide swath mode imagery, in-situ sea ice surface observations, weather station reports and the daily MODIS melt pond fraction and sea ice concentration dataset it was found that the MODIS sea ice concentration is negatively biased by 3 % and that the MODIS melt pond fraction is positively biased by 8 %. An investigation of the 8-day composite dataset of the MODIS melt pond fraction and sea ice concentration with regard to their seasonal development during late spring / early summer confirmed the existence of such biases. Hence, it was decided to apply these bias corrections suggested first by Mäkynen et al. [2014].

Minor Comments:

A: The text of the revised paper is adjusted with regard to all the suggested 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" 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. . ."

References

Eppler, D. T., L. D. Farmer, A. W. Lohanick, M. R. Anderson, D. J. Cavalieri, J. Comiso, P. Gloersen, C. Garrity, T. C. Grenfell, M. Hallikainen, J. A. Maslanik, C. Mätzler, R. A. Melloh, I. Rubinstein, and C. T. Swift: Passive Microwave Signatures of Sea Ice, in: Microwave remote sensing of sea ice (ed F. D. Carsey), American Geophysical Union, Washington, D. C, 47-71, doi:10.1029/GM068p0047, 1992.

Mäkynen, M., Kern, S., Rösel, A., and Pedersen, L.T.: On the Estimation of Melt Pond Fraction on the Arctic Sea Ice With ENVISAT WSM Images, IEEE T. Geosci. Remote, 52, 7366–7379, 2014.

Meissner, T., and F. J. Wentz: The emissivity of the ocean surface between 6 - 90 GHz over a large range of wind speeds and Earth incidence angles, IEEE Transactions on Geoscience and Remote Sensing, 50(8), 3004-3026, 2012.

Ulaby F. T., Moore, R. K., and A. K. Fung 1986. Microwave Remote Sensing. Active and Passive. Norwood, MA: Artech House Inc, 1986.

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

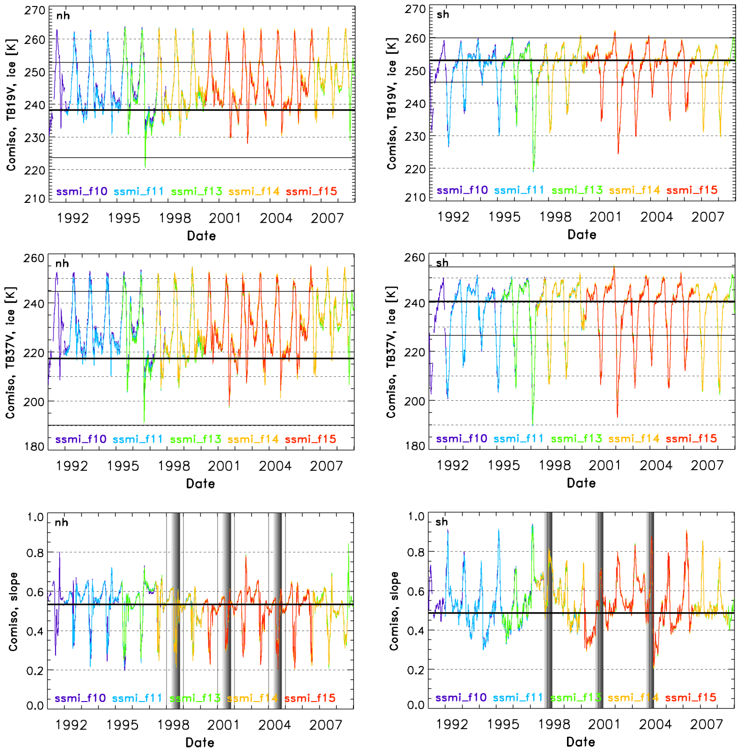

Fig. 1. Bootstrap F tie-points