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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 the Anonymous Referee #1 for the valuable comments that helped improving quality of the paper.

In the major comment the Reviewer points out some weaknesses of the presented dynamic tie-points approach. We will address them point-by-point first and then continue with the other comments (the answers are marked by A):

1. It would probably help to strengthen the entire chapter 4.5 by showing in more detail, how the retrieved SIC for single days changes when the dynamic tie points are used. How does it affect different regions?

A: The dynamic tie-points are only dynamic in time, not in space, so the effect on a single day map will be only through (small) offsets in the tie-point for water and ice, and thus small differences in SIC that will be hardly visible if shown as a map. The signature of ice including geophysical noise is not the same as it was in the past. Both sea ice extent and area and the geophysical noise parameters (sea ice emissivity, atmospheric parameters) have climatic trends. When computing sea ice climate record it is essential to ensure long-term stability and to avoid sensitivity to noise parameters with climatic trends. This is achieved with dynamic tie-points. Further, the dynamic approach is needed when accurately quantifying SIC uncertainties. In addition, it is an effective way of dealing with inter-calibration of satellite instruments and sensor drift. We will make the text clearer on this aspect (Sect. 4.5 and 5.6) and add horizontal lines corresponding to fixed tie-points in Fig. 8, so that the reader can see the effect of temporal variations of the dynamical approach as opposed to horizontal lines of the static tie-points.

2. According to my understanding, the suggested smoothing and averaging of TB for NT>95% areas just artificially removes the average 5% uncertainty that is being reported for NT. If not so, the authors need to discuss this in more detail.

A: The 15-day window for the tie-point average indeed causes smoothing, but the full range of NT SIC 95-100% is used (limited to a maximum of 5000 samples per day for the tie-points) to calculate the average. Then, the scatter of all selected NT>95% points (up to a maximum of 15 000 from all the swath files in the 15-day window) is used to calculate the tie-point uncertainty, which contributes to the total SIC uncertainty. The criterion for selecting the tie-points is to ensure areas with near 100% ice concentration. We are using NT, which has a different sensitivity to geophysical noise than Bristol. This point is not crucial and we could have used Bristol instead in the selection of the near 100% ice signature. However, by incorporating the NT SIC for this selection we also avoid "tuning" of the final hybrid algorithm too much towards the two algorithms that compose it, which is of particular importance in the marginal ice zone. The 15-day averaging period ensures that temporal regional noise, e.g. frontal systems, does not affect the tie-points and the ice concentrations estimates in other regions without such frontal systems on a day-to-day basis. Yet the 15-day averaging is sufficiently short to capture systematic seasonal changes happening on a hemispheric scale, e.g. the different stages of summer melt or fall freeze-up. The selection of only 5000 samples per day is to ensure that one day is not weighted higher than others when there are differences in the number of data-points from day to day. 5000 data points are easily achieved yet enough for a statistical mean. This will be clarified in the text (Sect. 4.5) accordingly.

3. Could a tie-point "recipe" (or a data base, i.e. monthly tie points instead of fixed values as provided in Table A1) be derived from the authors' research? Such an outcome would increase the impact of the presented research substantially.

A: The authors agree on the value of such details and will provide a step-by-step description of the dynamic tie-points retrieval (a new Appendix will be added) so that the results will be reproducible. As stated in the paper, tie-points will vary with calibration of the input data/version number and source, so our tie-points should not be used with other versions of the input data with potential different calibration. The "recipe" on the other hand can be applied to all versions/calibrations of the input data.

4. In the end, the paper runs a bit short in discussing this most innovative aspect. A more detailed discussion will definitely be an asset in this regard.

A: The discussion section (Sect. 5.6) will be strengthened (based on the answers provided above) accordingly in the revised version of the paper.

Detailed comments Ch1 P1272, L2: "globally" . . . rather: "polar regions"? A: Adjusted accordingly P1272, L3: Second sentence requires re-phrasing. A: Adjusted accordingly P1272, L12 and L14: "were" and "was" . . . change to "are" and "is" ... use present. A: Adjusted accordingly P1272, L29: remove "in turn" A: Adjusted accordingly P1272, L24: abbreviation SD is not introduced A: Adjusted accordingly

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P1272, L13 - P1274, L17: The listing of error sources is somewhat hard to follow. First, it is stated that there are two main error sources (emissivity variability, atmosphere). But then, more are introduced: thin ice, melt ponds. I suggest this paragraph to be rearranged or to prepend an enumeration of all error sources before the details are described. A: The paragraph will be rearranged P1272, L16: What are "internal properties"? A: This is the method applied to retrieve sea ice concentration from input brightness temperatures, which distinguishes each algorithm among the others. The text will be adjusted accordingly. P1272, L20: start new sentence after semicolon. A: Adjusted accordingly P1272, L27: specify what is meant by "tie-point signature". A: Predefined Tb for ice. The text will be adjusted accordingly.

Ch2 P1275, L10: "principle" is a bit fuzzy here. A: The channels: which ones and how the algorithms employ them – is meant here. The text will be adjusted accordingly. P1276, L28: The term "Round Robin Data Package" needs some additional explanation. A: Now introduced in the last paragraph of the introduction. P1278, L7: RRDP has already been introduced. A: Adjusted accordingly P1278, L16: PolarView and MyOcean need a reference (or a description) A: References added P1278, L19: change "got refrozen" to "refroze". A: Adjusted accordingly FIGURE1: Circles are hard to distinguish from squares in the present form. A: Fig. 1 and 2 will be adjusted accordingly P1279, L8-L11: I suggest that you indicate FYI, MYI as well as A and B types in the figure. A: Fig. 1 and 2 will be adjusted accordingly P1279, L14: I cannot see that OW pixels are mostly grouped within one point. I rather see a line as well. A: What was meant here is that the majority of the points is grouped around one point, however it is not easy to see from the plot, how many points are concentrated there and how many are spread along the line. The points stretching to the line are caused by atmospheric water vapor, liquid water- and ice clouds, surface temperature variability and surface roughening by wind. We will clarify this point in the text. P1279, L15: . . .also indicate the OW tie point. What is "geophysical noise"? A: The noise induced by geophysical parameters such as atmospheric water vapor, liquid water- and ice clouds, surface temperature variability and surface roughening by wind (all collectively called geophysical noise). This will be added to the text. FIGURE1: I think it would be beneficial to see the bootstrap 100% ice and OW lines in this figure. A: Fig. 1 and 2 will be adjusted accordingly. P1279, L25ff: Please indicate all the lines and points that you describe in the figure. Otherwise it is hard to follow your argumentation. A: Fig. 1 and 2 will be adjusted accordingly P1281, L27: “geophysical noise” see above. A: Adjusted accordingly

Ch3.3 The reader might wonder why the authors didn’t use MODIS SIC to evaluate their algorithms, at least for case studies.

A: We used MODIS sea ice concentration data for comparison, but only for the summer period to assess the algorithms performance over melt ponds. We did not do more as our primary focus was to evaluate high and low sea ice concentration data. We were not aware of a sufficiently quality-controlled MODIS sea ice concentration product to be used as a validation data set. For MODIS there is also the problem with cloud contamination, as the cloud filters developed for lower latitudes are not working that reliably in the polar latitudes. Moreover, identification of ice/water in the images depends on thresholds, which will take us back to the problem of dynamic tie-points. We will clarify the point in the revised version of the paper.

Ch3.4 P1282, L20-22: Some explanation is required here on how “large areas of 100% homogeneous thin ice” can be manually identified from ASAR data! A: “Visually” was meant rather than “manually”. By visual inspection (the same procedure as when producing ice charts), large homogenous areas of near 100% thin ice were identified as areas with a darker and homogenous texture. The text will be adjusted accordingly. Admittedly, visual interpretation comes with its own bias.

P1282, L26: “measurements” . . . rather “pixels”, or “data points”? A: Adjusted accordingly.

Ch3.6 P1284, L5: RRDP introduced again. A: Corrected. P1284, L12: . . . considered “the” following aspects. . . A: Adjusted accordingly.

Ch4.1 P1285, L3: remove parentheses. A: Adjusted accordingly. P1285, last line: Again, remove parentheses. Make a full sentence of this statement instead. A: Adjusted accordingly. P1286, L6-10: Why does the bias influence the ability to estimate the SD? This needs to be explained in more detail. A: More detailed explanation will be provided in the text. The large positive bias affects the SD in combination with a cut-off at 100% (which was not clear from the text before). For example, if real SIC is 75%, an algorithm with a positive bias of 20% will have average SIC of 95%, and by cutting-off all the values above 100% it reduces the scatter to only the values in 95-100% interval. For an algorithm with smaller bias and no cut-off the full scatter will be represented by SD. P1286, L10: intermediate OR high, intermediate AND high? Parentheses unclear. A: SIC > 75% is meant, only “high” will be kept. P1286, L18-19: Last sentence needs to be re-arranged. A: Adjusted accordingly. Figure3: Legend: Change “Stdev” to “SD” to be consistent. A: Adjusted accordingly.

Ch4.2. P1287, L12: State the coefficients in a full sentence, rather than in parentheses. A: Adjusted accordingly. P1287, L16-18. Be more specific in explaining why polarization and gradient ratios are less sensitive to surface temperature variations. A: The NT algorithm is based on polarisation and gradient ratios of Tbs, which more or less cancels out the physical temperature (Cavalieri et al. 1984). However, this is only true when there is only one surface temperature, so in cases of mixed ice types it may not be the case. With more different effective temperatures of fractions of the surface they do not all cancel out and we are left with a residual temperature effect in the ratio and thus in SIC inferred from the ratio. For the N90, in the case when emissivity of two ice types is similar, then a change in temperature will have same effect in both H and V channels. However, in the revised version of the paper we will remove this discussion, which was based on the correlation coefficients between SIC obtained by the PM algorithms and ice surface fraction from MODIS. We believe the correlation can be caused by at least two factors – effect of melt ponds and variations in Tb of the ice surface between melt ponds – and this study does not allow to separate the two.

Ch4.3 P1288, L7: Maybe the findings of Kwok et al. (2007) might be worth mentioning here (Kwok, R., J. C. Comiso, S. Martin, and R. Drucker (2007): Ross Sea polynyas: Response of ice concentration retrievals to large areas of thin ice, J. Geophys. Res., 112, C12012, doi:10.1029/2006JC003967). A: Adjusted accordingly. This reference will be also added to the Introduction.

Ch4.5 P1290, L5: "microwave emission". There has been a paper by Willmes et al. in 2014 (The Cryosphere, 8, 891-904, doi:10.5194/tc-8-891-2014) which investigated the microwave emissivity variability. Maybe their findings could be discussed in this context? (see also P1273, L13) A: This work will be cited in the Introduction and mentioned in the discussion section (Sect. 4.5). P1290, L8: Which data is the "two-week running window" applied to? Brightness temperatures? This suggestion needs some more explanation. It causes a smoothing in the input data that avoids an un-beloved scatter in the output data. Wouldn't it be more practical to stay with the scatter and use it for an uncertainty flag instead? As presented, the tie-point retrieval is dynamic in terms of season. Would it be useful to be also dynamic in terms of region? How large would regionally adjusted tie-point variations be in comparison to seasonal adjustments? A: The "two-week running window" is applied to the input brightness temperatures. Only selected data points are used, namely the ones where NASA Team algorithm gives SIC > 95%. The ice tie-point was subsequently calculated as average Tb value of these selected data points. The 15-days sliding window was mostly chosen for the OW end with the purpose of smoothing out the synoptic scales of weather perturbations. At the same time, the onset of ice emissivity changes due to snow melting should be reflected. We believe longer time windows will induce additional (too much) smoothing over the ice, and that shorter time-periods will introduce too much noise (over open water). However the scatter of all 15000 NT>95% points (from all the swath files in the averaging period) is used to calculate the tie-point uncertainty, which contributes to the total SIC per-pixel uncertainty. As for the dynamic tie-points in terms of region, the aim of this study is to identify a proper algorithm for climate dataset, which requires transparent description of techniques and uncertainties. It would be difficult to come up with proper uncertainty estimation in case we divide our region of interest - more or less arbitrarily - into sub-regions. One might argue that different tie-points for multiyear ice and first-year ice can still be used. However, computation of the uncertainty at the boundary of both regions will become problematic. How shall one treat mixed pixels? And - most importantly - one would need a validated quality-controlled ice type dataset spanning the entire period. We would recommend that regional (dynamic) tie-points would be an ideal tool for regional applications and for near-real time SIC retrieval of spatially limited areas but not for a climate dataset. This section (Sect. 4.5) will be made clearer in the text.

P1290, L14: Please specify what is meant by “inside monthly climatology of ice”. A: Within the limits of monthly climatology of sea ice extent. For the present study, we used a monthly climatology of maximum sea ice cover from NSIDC (http://nsidc.org/data/smmr_ssmi_ancillary/ocean_masks.html), and covering period 1979-2007. This climatology was then expanded by a distance of 350 km. This information will also be added to the revised manuscript.

Ch5.2 P1293, L9-10: How were the applied SIC thresholds (70% and 90%) chosen? A: Different weights were tested on the thin ice dataset. The optimal values were chosen so that the hybrid algorithm performs better over thin ice, and at the same time keeps its performance in other conditions at the same level as the original OSISAF algorithm. This will be also added to the text.

Ch5.3 P1294, L15-17: So is the chosen method feasible in this regard? A: Yes, the NT algorithm showed to be sensitive to melt ponds. The text will be adjusted.

Ch5.5 P1295, L18: “surface temperature” where does this information come from? A: It is the same surface air temperature at 2 m from ECMWF ERA-Interim as the one used earlier for correction over low SIC. It will be renamed to 2m-temperature in the text to avoid confusion. P1295, L19: “100%” SIC? A: Yes. It is added now. P1295, L20: “the atmospheric influence over ice is small”. . . is there are reference for this statement? A: The ERA Interim data we used showed that total water vapor and cloud liquid water content over ice were much smaller than over ocean. The atmosphere over ice is generally much colder than over the ocean, and cold air can contain much less moisture (including clouds) than warmer air. In addition, the emissivity is typically much larger for sea ice than for open water. Hence a change in the atmospheric water vapor of, e.g., 2 kg/m^2, imposes a different (smaller) change in the brightness temperature measured over sea ice compared to the one measured over open water (Oelke 1997). We believe the reason why the simple surface temperature correction did not work so well is that a) microwave radiation penetrates dry snow and partly also the sea ice and b) this penetration is a function of wavelength; accordingly different wavelengths penetrate to different depths in the ice and thus should encounter radiation based on different temperatures. This will be clarified in the text.

Ch5.6 P1295, L23: ". . .during the RRDP" needs re-phrasing. A: adjusted accordingly

Ch6 P1296, L22: Can an algorithm have "low sensitivity to the tie-points"? Would that be useful? A: Expressed inaccurately in the current text of the paper, this should be: "an algorithm can be less sensitive to uncertainties in tie-points". Low sensitivity to tie-points in general is not necessarily a good thing, since it is the tie-points that allow us to compute SIC.

P1296, L19 (1 of 2): Which are the error source that cannot be correct for? According to my understanding, none is corrected for in the presented research but an algorithm setup with the lowest sensitivity suggested. A: More precisely: "The error sources that cannot be corrected for by the atmospheric correction procedure suggested". By the error sources that cannot be corrected for we mean cloud liquid water and precipitation – these are found to be less reliable in ERA Interim and thus the suggested atmospheric correction will not be optimal for these. This is both found in literature (Andersen et al. 2006) and confirmed by our work. We address this in the Sect. 3.5. In the present research we correct for atmospheric and surface effects using a Radiative Transfer Model (RTM) (Wentz (1997)). Fields of 10m-wind speed, total columnar water vapor, and air temperature at 2m from the ECMWF ERA-Interim Numerical Weather Prediction (NWP) re-analysis are used in this process. The correction is described in the Sect. 3.5, the results are presented in the Sect. 4.4. and the effect of the correction is demonstrated by Fig. 7. In the revised version of the manuscript we will make the point clearer.

P1296, L19 (2 of 2): A dynamic tie-point retrieval could provide a correction for sensor drift, inter-sensor differences and maybe emissivity variations. But this is not what is being achieved with the suggested data smoothing. This issue could be pointed out more clearly. A: The reason for the 15-days smoothing is only to reduce noise in the tie-points, especially the ones for open water, as there is quite a lot of variation from day to day. It is the longer-term variations that we want to correct for. The main point of the dynamic tie-points is the fact that the tie-points follow the seasonal cycle of signatures including the atmosphere.

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