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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.

Anonymous Referee #1

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The per-area fraction of sea-ice is an important parameter for climate monitoring and an essential forcing variable for climate models. However, the long time series of the required microwave brightness temperatures are subject to inter-sensor calibration issues and potential climatic trends in atmospheric water vapour, water surface roughening and surface emissivity.

The authors present a very interesting and comprehensive overview on thirty different techniques for the retrieval of sea-ice concentration from microwave satellite data. They group all available algorithms into 13 different categories and investigate the sensitivity of the retrieval precision and accuracy within each group to different error sources. The advantages and drawbacks of the algorithm categories are discussed in form of

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an inter-comparison. The presented study aims at identifying the potential impact of these error sources on the sea-ice concentration product and derived quantities. In the end, the article aims for a conclusion that points towards an optimal algorithm choice.

Overall comment

The paper presents excellent and important research that certainly merits publication. However, I think that some issues should to be addressed before the paper is accepted in its final form. In some parts, an elucidation of research implications would be beneficial and the description of procedures could be more focused. The discussion can be strengthened to some extent, especially with regards to the “optimal algorithm and challenges”.

Major comment

The authors suggest tie-points for SIC retrievals to be derived dynamically. They present very good research which could help to find a concrete formulation of such an approach. However, the presented dynamic tie-point retrieval has some weaknesses that are not yet sufficiently discussed. 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? 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. 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. 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.

Detailed comments

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Ch1

P1272, L2: “globally” . . . rather: “polar regions”?

P1272, L3: Second sentence requires re-phrasing.

P1272, L12 and L14: “were” and “was” . . . change to “are” and “is” ... use present.

P1272, L29: remove “in turn”

P1272, L24: abbreviation SD is not introduced

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.

P1272, L16: What are “internal properties”?

P1272, L20: start new sentence after semicolon.

P1272, L27: specify what is meant by “tie-point signature”.

Ch2

P1275, L10: “principle” is a bit fuzzy here.

P1276, L28: The term “Round Robin Data Package” needs some additional explanation.

P1278, L7: RRDP has already been introduced.

P1278, L16: PolarView and MyOcean need a reference (or a description)

P1278, L19: change “got refrozen” to “refroze”.

FIGURE1: Circles are hard to distinguish from squares in the present form.

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P1279, L8-L11: I suggest that you indicate FYI, MYI as well as A and B types in the figure.

P1279, L14: I cannot see that OW pixels are mostly grouped within one point. I rather see a line as well.

P1279, L15: . . .also indicate the OW tie point. What is “geophysical noise”?

FIGURE1: I think it would be beneficial to see the bootstrap 100% ice and OW lines in this figure.

P1279, L25ff: Please indicate all the lines and points that you describe in the figure. Otherwise it is hard to follow your argumentation.

P1281, L27: “geophysical noise” see above.

Ch3.3

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

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!

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

Ch3.6

P1284, L5: RRDP introduced again.

P1284, L12: . . . considered “the” following aspects. . .

Ch4.1

P1285, L3: remove parentheses.

P1285, last line: Again, remove parentheses. Make a full sentence of this statement instead.

P1286, L6-10: Why does the bias influence the ability to estimate the SD? This needs to be explained in more detail.

P1286, L10: intermediate OR high, intermediate AND high? Parentheses unclear.

P1286, L18-19: Last sentence needs to be re-arranged.

Figure3: Legend: Change “Stdev” to “SD” to be consistent.

Ch4.2.

P1287, L12: State the coefficients in a full sentence, rather than in parentheses.

P1287, L16-18. Be more specific in explaining why polarization and gradient ratios are less sensitive to surface temperature variations.

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).

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)

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

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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?

P1290, L14: Please specify what is meant by “inside monthly climatology of ice”.

Ch5.2

P1293, L9-10: How were the applied SIC thresholds (70% and 90%) chosen?

Ch5.3

P1294, L15-17: So is the chosen method feasible in this regard?

Ch5.5

P1295, L18: “surface temperature” where does this information come from?

P1295, L19: “100%” SIC?

P1295, L20: “the atmospheric influence over ice is small”. . . is there are reference for this statement?

Ch5.6

P1295, L23: “. . .during the RRDP” needs re-phrasing.

Ch6

P1296, L22: Can an algorithm have “low sensitivity to the tie-points”? Would that be useful?

P1296, L19: 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 dynamic tie-point retrieval could provide a correction for sensor drift, inter-sensor differences and maybe emissivity variations.

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But this is not what is being achieved with the suggested data smoothing. This issue could be pointed out more clearly.

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