The Cryosphere
Discussions

Interactive comment on "Measuring sea ice concentration in the Arctic Ocean using SMOS" *by* Carolina Gabarro et al.

Anonymous Referee #2

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Observation of sea ice concentrations is a highly relevant topic. The support of currently used sea ice concentration retrieval algorithms from other passive microwave sensors operating at higher frequencies by using L-band observations from SMOS is much appreciated. Especially during summer conditions where common products yield higher uncertainties a low frequency algorithm is very welcome. The Manuscript is mostly well written and tries to combines theoretical and empirical aspects to derive sea ice concentration from multi angular observations from SMOS. A solid statistical analysis of the estimated result is given and compared to a sophisticated operational sea ice concentration product. This manuscript is suitable for publication in The Cryosphere after addressing the following comments.

General comments:

1. You employ a physical emissivity model where you find AD and PD relevant but TB too much affected by thin ice. AD and PD are not much affected in the physical model but in the end in the SMOS data the thin ice degrades your retrieved sea ice concentrations a lot. One could argue that your emission model is not able to describe the observations adequately. At this point the question arises if TB would not be even a better indicator for sea ice concentration. It would fortify your approach using the angular difference if you compare the retrieval to a simple TB based approach with the same tie points to show that there is additional information on the ice concentration in the AD compared to TB.

2. During the course of the paper you use different concepts of describing microwave emission which lead to confusion. Firstly you start with emissivity in Eq. 1 and introduce it as 1-reflectivity where the reflectivity is defined for each layer transition while the emissivity should characterize the overall emission. You also use the term "signatures" somewhere in addition to describe MW emission. This could need some clarification.

3. When using the angular difference, you connect data with quite different footprint sizes maybe about 25km vs 60km because of the 35 degree incidence angle difference. I guess this can influence your product at the ice edge and anywhere where you have mixed surface types and should be somehow discussed.

Specific:

P1, L3: remove "interferometric"

P1, L19-21: there are plenty of observations and algorithms observing sea ice and sea ice decline, you cite some of those dataset. Thus this statement is confusing.

P1, L24-25: all the listed publications are sea ice related, thus I would add an important one: Tian-Kunze, X., Kaleschke, L., Maaß, N., Mäkynen, M., Serra, N., Drusch, M., and Krumpen, T.: SMOS-derived thin sea ice thickness: algorithm baseline, product specifications and initial verification, The Cryosphere, 8, 997-1018, doi:10.5194/tc-8-

997-2014, 2014.

P1, L26: add instrument name MIRAS

P2, L5: extension–>extent

P2, L17: add "frequencies" before .

P3, L20: specify which outliers are filtered out, where are they coming from?

P3, L22-23: define "bottom of the atmosphere" and your applied correction for that

P3, L26-27: you write you interpolate TB to locations using a polynomial fit. It is not clear to me if this is a spatial operation or a point wise operation interpolating missing incidence angle ranges in the TB-incidence angle-space.

P4, L6-7: it is not clear for what the NIC data is used

P4, L27-20: The sentence is quite confusing; You say the "latter" which, if I read it correctly, means the dielectric constant is dependent on the incidence angle and thus becomes a tensor. Or do you mean the reflectivity changes with incidence angle (like described by the Fresnel Equations)?

P4, L32: define "standard Arctic temperatures and salinity values"

P5, L3: sensitivity–>variation?

P5, L3: it is unclear for what the reference is there

P5, L8-10: This is a bit confusing, why do you need a constant thickness of the snow layer in an incoherent model (Eq. 2) when the absorption in the snow is negligibly small? Also the mentioning of kappa_e and SSA is confusing here.

P5, L11: remove "spontaneous"

P5, L11-13: Actually the water under sea ice has a contribution to the emissivity, as you can easily calculate with your model, but you mean probably that the emissivity is not

getting higher with increasing ice thickness from about 60cm, i.e., the signal saturates. I would rephrase the sentence.

P5, L17: I cannot find anything related to your sentence in the reference you are giving here.

P5, L23-24 (Eq. 2): I cannot see how infinite layer reflections are accounted for. Also that the physical snow temperature times 1-reflectivity of snow-air boundary is simply added is unphysical and must be an error in the equation.

P5, L31-32: remove "conducting". For sure it is also true for a conducting medium but you stated the alpha for low-loss-medium, means no- to low- conducting material.

p6, L1: The dry snow permittivity is actually known to be density dependent, see for example: C. Matzler. Microwave permittivity of dry snow. IEEE Transactions on Geoscience and Remote Sensing, 34(2):573–581, Mar. 1996. ISSN 01962892. doi: 10.1109/36.485133. and M. Tiuri, A. Sihvola, E. Nyfors, and M. Hallikainen. The complex dielectric constant of snow at microwave frequencies. IEEE Journal of Oceanic Engineering, 9(5):377–382, Dec. 1984. ISSN 0364-9059. doi: 10.1109/JOE.1984.1145645.

Eq. 4, Eq. 5, and Eq. 6: I would give the coefficients or skip the equations.

P6, L17: remove "model value necessary for the" or rephrase

P6, L20-23: I don't understand the sentence. The water under the sea ice does not decrease the emissivity of ice but has a fundamental contribution to the emissivity (See also comment on P5, L11-13). I see in Fig. 5 only that emissivity of sea ice increases with ice thickness but not that water under the ice decrease the emissivity of ice. Also: The four layer model does not come with an equation as Eq. 2 only describes ice, snow and air.

P7, L2-3: sentence is confusing, please elaborate or clarify.

P7, L6 (Eq. 7) you should indicate the incidence angle dependence of PD, TBh and TBv

P7, L24: "and snow"–>"with snow cover"

P8, L1-2: add "as described by our model"

P8, L7: remove ", which are rarely available,"

P8, L9: theoretical–>"modeled"

P8, L9-10. The partial derivatives will strongly depend on where they are evaluated as the quantities are nonlinear. This should be mentioned or accounted for. Also the dynamic range of the measurement needs some more explanation.

P8, L21-25: the discussion would need the inclusion of the evaluation point of the partial derivatives

P8, L29: remove "unambiguously", these retrievals also have an uncertainty.

P9, L4: "radiometric values"–>"brightness temperatures"?

P9, L13: add "which" behind first comma

P9, L13: "suggest"–>"suggests"

P9, L14: "maps"–>"retrieval"

P9, L23: remove "algorithm" or rephrase

P11, L11: "extension"–>"extent", "maximum"–>"close to its annual maximum"

P11, L13-14: Thin ice time period was not used for the tie point? this comes as a surprise since your emission model suggested that your key parameters/indices are not sensitive to ice thickness. From where is it known that thin ice introduce a bias in your SIC retrieval, reference?

P12, L3-7: you should mention that "theoretical" means "modeled using Eq. 2"

C5

P12, L25: "adn" –> "and"

P12, L25-26: reference for penetration of frequencies used by OSI-SAF

P12, L28: why are TBs important if your retrieval uses AD?

P13, L19: the referenced figure F. 15 says "correlation coefficient" on the y-axis, so what is really shown?

P14, L3: I could not find this statement in Section 3, SIC is not discussed in Section 3

P14, L16: "of"–>"for"

P14, L18: "changes in the physical media" –> "exchange of the physical medium" or be more specific and write directly about open water and sea ice

P15, L11: I don't understand the sentence: what is meant by "single point viewed"

Fig. 13: would be easier to interpret if the time period with summer tie points is marked or at least mentioned in the caption