

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

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Authors: We would like to thank both referees for the interesting and useful questions and improvements suggested.

Anonymous Referee #2

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 combine theoretical and empirical aspects to derive sea ice concentration from multi angular observations from SMOS. A solid statistical analy-

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sis 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.

AUTHORS: The theoretical models predict that AD and PD are preferable to TB in order to retrieve SIC, not because the larger sensibility of TB to thin ice but also to the other geophysical parameters (temperature, salinity). This does not mean that AD and PD are unaffected by ice depth, as later confirmed in the experiments, but they are still more robust than plain TB (see table 3). The problem with thin ice needs to be attacked, indeed, and for that goal a multiparametric retrieval (both SIC and ice thickness) is in order, as commented in the Conclusions; however, this study goes beyond of the scope of the present paper, and will be addressed in a future work.

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.

AUTHORS: Agreed. The sentence has been rephrased. We have specified that e

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is 1-reflectivity for a unique layer. Latter, we explain how to compute the brightness temperature (not emissivity, which has been changed) for a two-layer model. The word 'signature' has been replaced by 'emissivity' or 'emission'.

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.

AUTHORS: The reviewer is completely right: At 25° incidence angle SMOS resolution is around 38 km and at 60° is around 70 km, or an increase of 84%. So certainly the measurements do not refer to the same area, and this is why probably the use of AD is better suited for cases in the interior areas and is more problematic close to the coast. A comment on this issue has been added in the text.

Specific:

P1, L3: remove "interferometric"

AUTHORS: Done.

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.

AUTHORS: Agree. The last two sentences have been deleted.

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.

AUTHORS: Now included.

P1, L26: add instrument name MIRAS AUTHORS: Done in page 2 .

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P2, L5: extension→extent AUTHORS: Done.

P2, L17: add "frequencies" before .

AUTHORS_ Done .

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

AUTHORS: We filter out all the Tb measurements, from each grid point, which are further away than $3 \cdot \sigma$. It is added in P3L23 of the new version.

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

AUTHORS: This sentence was wrong. The final TB is taken at the reference frame "bottom of the atmosphere", when atmospheric and geomagnetic and ionospheric issues are already corrected for. We have rephrased the sentence.

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.

AUTHORS: We interpolate the SMOS data of the same grid point (pixel) to obtain the TB in the incidence angles which are missing.

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

AUTHORS: It is now explained in line P4L7.

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)?

AUTHORS: Agree. The sentence was confusing. We have made an effort to make it clearer.

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

AUTHORS: This information is now added in the manuscript.

P5, L3: sensitivity→variation?

AUTHORS: Done.

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

AUTHORS: It was referred to the snow effect on the SMOS TB. Now the text has been modified to make it clearer.

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 κ_e and SSA is confusing here.

AUTHORS: Agreed. This part is deleted in the new version. This is true for any media, but has no sense when snow media is in the middle layer.

P5, L11: remove "spontaneous" AUTHORS: Done.

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.

AUTHORS: Done.

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

AUTHORS: It was a mistake, the reference Maa β et al. 2015 has been deleted 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.

AUTHORS: The reviewer is right, the equation as written in the paper was wrong, and

the infinite reflections were not taken into account there. We apologize for the mistake; it is now properly written.

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.

AUTHORS: Done.

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.

AUTHORS: Agreed. The paper has been modified accordingly, including the appropriate citations.

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

AUTHORS: The authors prefers to keep the equations on the manuscript, since they shows the dependences to other parameters. However, we prefer not to add the values of the coefficients, since they do not bring any additional information to the readers, and all the values are in the cited papers.

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

AUTHORS: Agreed. Done.

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:

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The four layer model does not come with an equation as Eq. 2 only describes ice, snow and air.

AUTHORS: The sentence was not correct, actually. The decrease of emissivity is due to the reduction of the ice thickness. This sentence has been corrected.

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

AUTHORS: Certainly the sentence was confusing. We have deleted it since the information we wanted to transmit here is already given in the same paragraph in: 'It is possible, however, to define a number of indices combination of brightness temperature observations that are less sensitive to the unknown physical parameters. '

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

AUTHORS: Agreed. Done.

P7, L24: "and snow"→"with snow cover" -> Authors: Done.

P8, L1-2: add "as described by our model" -> Authors: Done.

P8, L7: remove ", which are rarely available," -> Authors: Done.

P8, L9: theoretical→"modeled" -> Authors: Done.

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.

AUTHORS: The sensitivities, listed in table 2, are obtained using the following range of parameters, for sea water : $T_{sea}=[2,15]$, $S_{sea}=[10,38]$ and for ice: $T_{ice}=[-20,-5]$ and $S_{ice}=[2,12]$. This information is added in page 8.

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

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AUTHORS: This is addressed in the previous answer.

P8, L29: remove "unambiguously", these retrievals also have an uncertainty. -> AUTHORS: Completely agreed. Done.

P9, L4: "radiometric values"→"brightness temperatures"? -> AUTHORS: Done.

P9, L13: add "which" behind first comma ->AUTHORS: Modified

P9, L13: "suggest"→"suggests" -> AUTHORS: Done.

P9, L14: "maps"→"retrieval" -> AUTHORS: Done

P9, L23: remove "algorithm" or rephrase - > Authors: Done

P11, L11: "extension"→"extent", "maximum"→"close to its annual maximum"-> AUTHORS: Done

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?

AUTHORS: The models do not suggest that the indices are NOT sensitive, they suggest that the sensitivity of the indices to thin ice is lower than using TBs. Figure 5 and table 2 show that there is, still, a sensitivity of AD and PD to thin ice, even though this is smaller than TB. The sentence has been modified according.

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

P12, L25: "adn" → "and" ->AUTHORS: Done.

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

P12, L28: why are TBs important if your retrieval uses AD? -> AUTHORS: True, it has

been modified.

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

P14, L3: I could not find this statement in Section 3, SIC is not discussed in Section 3-> AUTHORS: True, we have modified 'SIC' by 'TB'.

P14, L16: "of"->"for"-> AUTHORS: Done.

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 -> AUTHORS: Done.

P15, L11: I don't understand the sentence: what is meant by "single point viewed" -> AUTHORS: It has been rewritten now.

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

[Interactive comment on The Cryosphere Discuss.](#), doi:10.5194/tc-2016-175, 2016.

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