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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 1 The use of L-band data for deriving SIC especially during summer, is indeed very welcome and relevant.

During summer traditional sea ice concentration (SIC) algorithms using higher frequency channels (19- 90GHz) have high uncertainties because the higher frequency brightness temperatures (Tb's) are affected by emissivity variability in the snow/sea ice surface fraction and because of higher levels of water vapor and cloud liquid water in the atmosphere than in winter. L-band Tb's are less sensitive to both noise sources (than 19-90GHz Tb's). In addition SMOS is measuring at several incidence angles at

every point which can be exploited in the SIC retrieval (as it was done in this study).

So the idea is good, however, this MS is a collection of elements and sections which are not well integrated and focused towards the actual aim of retrieving SIC: for example, the sea ice forward model is used for selecting the parameters AD and PD to use for SIC retrieval and for estimating uncertainties, however, this may have been more convincing using measurements, or at least a combination of simulations and measurements. This could also give credibility to the model simulations.

AUTHORS: We have done our best to improve the manuscript along the lines proposed by the reviewer. First a theoretical analysis has been done based on forward models (section 3 and 4.3). On the other hand, in section 4.4 we have computed the parameters based on SMOS measurements. Finally in section 5, we conclude that based on the performance of the different algorithms, it is better to use only the AD index than both AD and PD. Therefore we combine theoretical considerations based on models and measurements in the manuscript. However, we cannot compute uncertainties with SMOS measurements, since the database which is used to validate the SIC data (called RRDB, p. e. Ivanova, et al., 2015) is previous to the SMOS launch.

Several assumptions needs supporting references or investigations (using data), for example, the postulate that Sic is a linear function of AD and PD and that the summer sea ice tie-point is derived from data which are at 100% SIC.

AUTHORS: The referee is right, and that part has now been correctly framed: we do not intend to use a linear model, but to estimate the average slope. Reviewer is right remarking that the computed summer sea ice tie-point probably does not correspond to 100% ice. OSI-SAF SIC data and National Ice Center ice Charts information are used to determine the region with 100% ice. However, OSI-SAF SIC presents large errors during summer (Ivanova et al. 2015), and this could produce errors on the computation of the SMOS 100% SMOS tie point. This is a clear limitation of the method; a short discussion has been added to the text.

Specific comments:

P1, L19-20: SIC as an essential climate variable (ECV) has perhaps the longest continuous time-series of satellite measurements among all ECV's and its decline is measured in detail. I don't understand what is meant by lack of observations. You need to specify that. What is meant by quasi-continuous? Please reformulate or delete the sentence.

AUTHORS: Misleading and wrong sentence, we have deleted it.

P2, L22: It is important to state here that fig. 1 is for open water (it is not mentioned!). Fig. 1 could be relevant for discussion of the open water tie-point. Please include it in the discussion or delete it. The open water (L-band) emissivity is in addition to salinity also sensitive to SST and wind-speed.

AUTHORS: Agreed, it has been specified in the text.

P2,L25-26: Please mention the range of frequencies so that this is clear in the sentence. Even though the MY and FY ice Tb's at L-band are overlapping there may be some differences in the mean value. This is a problem for the SIC retrieval since you may be introducing an ice type SIC bias. It needs to be shown, using measurements, how you handle this.

AUTHORS: The frequency range has been added now in the manuscript, as suggested. Regarding the potentially different radiometric behaviour of MY and FY, we have verified that the difference in the mean values is very small, of around 0.02% (obtained from measurements). Certainly the standard deviations of the two ice types are different: the STD of FY is the double of that of MY ice. But since the mean value is almost the same for MY and FY we do not expect any ice type SIC bias; what should be expected is an increase in uncertainty when FY is dominant, that is, that the error in SIC estimate is larger in that case.

P2,L31: delete "quasi" -> AUTHORS: Done

P2,L35: delete "theoretical" -> AUTHORS: Done

P3,L20: Is the galactic reflection correction applied or not?

AUTHORS: No, the galactic reflection is not significant at high latitudes (as explained paragraph 2 from section 2.1), and is not corrected.

P3L23: It is unclear what is corrected at the bottom of the atmosphere (surface?) using what?

AUTHORS: The geomagnetic and ionospheric rotation and the atmospheric attenuation are corrected to get the bottom of the atmosphere TB. Moreover, some points with low accuracy have been eliminated, such as aliasing (Camps et al., 2005), Sun reflections, and Sun tails. Now the last sentence of the paragraph is deleted, which we think was quite confusing.

P3L26: What is the full range of incidence angles? Are they also extrapolated?

AUTHORS: The full range of incidence angles is from 0° to 65° (written at the end of section 1). We do not extrapolate data.

P3L30: Add "for comparison" at the end of the sentence. -> AUTHORS: Done

P4L2: Add "dynamic" after "monthly". -> AUTHORS: Done

P4L19: replace "observing" with "electromagnetic" -> AUTHORS: Done.

P4,eq.1: Eq. 1 is describing the self-emission of a homogeneous and isothermal material and there is no term for atmospheric reflection or emission/scattering. Perhaps less important terms at L-band but worth mentioning.

AUTHORS: We have added a new equation 1 in the manuscript which describes the effect of the atmosphere on the final TB, as suggested.

P4L23: Debye, 1929, maybe there is a more sea ice relevant reference? Deleted, the reference had not sense here.

AUTHORS: This reference had not sense here and has been removed.

P4L23: Add: "...depend on the incidence angle and..."-> AUTHORS: Done

P4L24: replace "complex value" with "complex number" -> AUTHORS: Done

P5L18: "decrease" or "increase"? please give reference. -> AUTHORS: Right, it should be 'increase'. Changed in the manuscript.

P8L18: reference for the physical parameters variability needs to be given. Only these three parameters contribute to the budget? Could perhaps also mention snow cover, sea ice type...

AUTHORS: Theses values are just the typical ones, defining the range of variability for them. We do not refer to other parameters (snow cover, sea ice type) since temperature, salinity and ice depth (d) are the only parameters that play a role in our emissivity model. This is now explained in more detail in the paper.

P9L29: SIC as a linear function of AD and PD. You need to show that this is true (using measurements), otherwise you will have to build in the non-linearities into the SIC model.

AUTHORS: Corrected, as commented above.

P11L3-4: why "less prone to errors"? and what is "natural way"?

AUTHORS: The sentence has been deleted. In section 4.5 the characteristics of the MLE technique is described, so it is not necessary to say anything else here.

P13L31: Sensitivity to physical temperature. This might be true for some algorithms but not all, please give a reference.

AUTHORS: We have referred to Ivanova et al. 2015 in this context, and explained in the text now which bands and algorithms suffer of this problem.

P14L1: "sensitive" -> "sensitivity" -> AUTHORS: Done

P14L11: The advantage.. this sentence is nonsense. All tie-points are derived empirically and static tiepoints are prone to errors due to sensor drift or seasonal variability, geophysical and climatic trends (in the noise).

AUTHORS: This sentence has been deleted.

P14L30: (less noisy) than what?

AUTHORS: Than the linear inversion. This sentence has been clarified.

P15L1: what is meant by "good" correlation? Please quantify.

AUTHORS: Sentence has been improved and values are now given

P15L13: The AD and PD ice- water contrast is not high for all incidence angles. The Tb contrast is high at all incidence angles. The dynamic range (between ice water) using Tb is much higher than AD and PD and therefor they could be less noisy. Please explain.

AUTHORS: Although the dynamic range of TB (when comparing ice and water) is larger than those from AD and PD, AD and PD are less sensitive to other parameters (and so less affected by their natural variability), i.e. the ice thickness, the physical temperature and salinity of the Ice/water (refer table 2 and 3). This means that the capability of TB to discriminate between different states is smaller, due to the greater dispersion of the geophysical response, than that of AD and PD, what makes the later better suited to derive SIC.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2016-175/tc-2016-175-AC1-supplement.pdf

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