

## ***Interactive comment on “The Arctic Ocean Observation Operator for 6.9 GHz (ARC3O) – Part 2: Development and evaluation” by Clara Burgard et al.***

**Clara Burgard et al.**

clara.burgard@hzg.de

Received and published: 30 April 2020

RC: Reviewer Comment, AR: Author Response

RC: Reviewer Summary: The manuscript assesses sources of uncertainty of brightness temperature from 6.9 GHz observations at top of the atmosphere. The brightness temperature was simulated using a scheme developed for this purpose, called the Arctic Ocean Observation Operator (ARC3O), which is described in details in the manuscript. This is also called observation operator as appears in the title. It comprises an earth system model with its atmospheric and oceanic components. Results on the difference between the simulated and observed brightness temperature are presented

Printer-friendly version

Discussion paper



with detailed study of the factors that contribute to the differences, including the source of the assimilated ice concentrations (three sources are tested). The study presents results in three sections to address ice conditions during cold winter, onset of melt in the spring and melting stage in summer. The contribution of the ocean and atmosphere parametrization are presented in a separate section. The manuscript presents good and timely piece of work. Some results are very much needed in order to proceed with more accurate ice monitoring and modelling. An example is the effect of melt pond on brightness temperature. Also, the finding that variability in ice concentration estimates is the main driver for brightness temperature. Such findings set priorities for further research by both modeling and parameter retrieval communities. I find the methodology well-planned; the manuscript is well-organized and written, graphs are well prepared and the conclusions are clear (though they can be grouped and summarized better in the Conclusions sections). This is the first study (as far as I know) that uses this simulation approach to assess the uncertainty of the microwave radiometric observations. I recommend publications after a revision that addresses the following concerns.

AR: Thank you very much for the positive feedback, and for your detailed, constructive comments on how to further improve our manuscript. We plan to address all your comments as described in the following.

—

RC: In the Abstract... you do not really “evaluate” ARC3O. I see that you use this tool to evaluate the uncertainty of the brightness temperature and relate it to the contributing factors from ocean/ice and atmosphere. If this is true please re-phrase line 4 in the Abstract.

AR: Our setup does indeed not allow us to evaluate ARC3O directly. However, through understanding the uncertainty of the brightness temperature and its drivers, we indirectly evaluate the results of ARC3O. We will reformulate this to clarify.

—

RC: P1 L7-8 “We find that they differ up to 10 K in the period between October and June, depending on the region and the assimilation run”. I understand that the 3 runs differ by up to 10K. But what are differences with AMSR-E observations?

AR: We apologize for the confusion. We mean that the individual runs differ by up to 10 K from AMSR-E observations. We will reformulate this to make that clearer.

—

RC: P2 L2: “by the physical noise at the level of the satellite”. What is physical noise? Do you mean electronic noise?

AR: Yes, we mean electronic noise and now say so in the manuscript.

—

RC: P2 L4: “relevant climate variables”. Do you mean physical variables? I think the list of “climate” information in the next 2 lines are physical parameters of the snow-covered ice.

AR: Thank you for pointing out this imprecision. It is true that we only describe physical properties of the snow-covered ice (having in mind that these are the important ones for 6.9 GHz). However, for higher frequencies, atmospheric information becomes important as well. This is what we hinted at. We will reformulate to clarify.

—

RC: P2 L19: “Additionally, the climate system as a whole can be evaluated with this approach and not only individual variables”. Please clarify.

AR: Here, we mean that the translation from model state into brightness temperature requires more than one physical parameter. This way, the evaluation of the simulated brightness temperature is in fact an evaluation of the physical climate state as given by the combination of several variables instead of an evaluation of an individual variable taken out of the climatic context. We will reformulate to clarify.

[Printer-friendly version](#)[Discussion paper](#)

—

RC: P2 L24: the promise made in the statement “While we focus on the frequency of 6.9GHz in this study, the framework proposed here...can be extended to investigate the simulation of brightness temperatures at other frequencies...” is offered without a substantial argument. Knowing the complexities of the microwave/snow/ice interaction at frequencies higher than 19 GHz, I would be in doubt about this promise.

AR: We agree that the exact same methodology cannot be used as easily for higher frequencies due to the challenges of representing snow accurately. However, we believe that further research and revisiting old and new in-situ measurements can help us to simplify some of the governing snow properties. And, using an idealised setup similar to the setup we used in the companion manuscript of this manuscript, we might find a simplified way of describing snow properties relevant to the simulation of brightness temperatures. Again, we agree, this could not be done within the next few months. But maybe in the next years to decades? We will add such disclaimer.

—

RC: P3 L20: “theoretical satellite”?? why “theoretical”?

AR: We chose to write "theoretical" because a satellite will never be able to rotate around a climate model, which is a theoretical construction. We realize that the information about the climate model is missing in this sentence. We will remove "theoretical" and clarify that our observation operator is defined to be applied to climate model output.

—

RC: P3 Eq. 1: the use of this equation should be declared here. I am not sure how and where this equation is used. Don't you use MEMLS to calculate surface brightness temperature?

AR: We apologize for the confusion. We will move the equation to a later point, where

[Printer-friendly version](#)[Discussion paper](#)

we describe the emissivity we use for wet snow, to clarify the relationship between emissivity and brightness temperature.

—

RC: P4 the flow chart of Fig.1 is well-presented. But does the RTM need “bulk” snow temperature? This is not mentioned in the box of GCM. Also, what is the water vapor in this box? Atmospheric?

AR: The atmospheric RTM does not need the bulk snow temperature, but rather the surface temperature of the snow-ice column. Combined with the ocean surface temperature, this snow-ice column surface temperature gives us an approximation of the mean air temperature near the surface. All other information about the snow-ice column is already comprised in the ice brightness temperature computed by MEMLS. The water vapor is atmospheric. This will be clarified.

—

RC: P6 Equations 2 and 3: please mention the basis of these equations...empirical? Then based on what data? Or perhaps from a physical model?

AR: These equations are a fit based on the results of the physical model SAMSIM (Griewank and Notz, 2015), which describes the evolution of salinity in sea ice in a 1D setup. The model results were compared to observational data for evaluation. We used these equations for the "salinity as a function of depth" in our companion manuscript. We will add a few words about the origin of these equations in the manuscript.

—

RC: P6 L13: In equation 4 and the definitions of its terms: it is strange to find the term of brine salinity in the equation of the density of seawater (inserted in line 15). Also, the brine volume fraction is defined in terms of “S” but “S” is not defined as the salinity of the ice layer. The definition of brine volume fraction is not convincing. Did you switch the numerator and denominator?

[Interactive  
comment](#)

[Printer-friendly version](#)

[Discussion paper](#)



AR: We apologize for the confusion. The brine liquid is defined as "seawater" here because it is a liquid with a similar chemical composition as seawater (since it is a result of the freezing process of seawater). And the salinity of this liquid is brine salinity. This is why we use brine salinity in that equation.  $S$  is the salinity of the ice layer, depending on the depth (see Eq. 2 and 3). The brine volume fraction is defined following the equation given in Notz (2005), we did not switch numerator and denominator. To avoid confusion, we will restructure this part.

—

RC: P7 L10: "we assume that the melting snow emissivity is 1...". Not sure that this assumption is reasonable. The microwave emissivity should be significantly lower than 1. Since the work has already been done using this assumption, the authors may include one line to justify or comment on this assumption.

AR: At 6.9 GHz, the emissivity of wet snow is very high (Hallikainen et al. 1986, IEEE on Antennas and Propagation, Lee et al. 2018, JGR: Atmospheres). Experiments conducted by one of our co-authors also show an emissivity of wet snow near 1. We will include the reference in the manuscript.

—

RC: P7 L11: the use of Eq. 1 is mentioned here for the first time. Please clarify in terms of the use of MEMLS.

AR: We use Eq.1 here because in the case of wet snow we cannot use MEMLS due to a lack of information about the liquid water content in the snow. Instead, we rely on the relationship between temperature and emissivity, setting the emissivity to 1. As explained in a previous answer, we will define and clarify the use of Eq.1 in this part.

—

RC: P8 Fig.2: This an interesting data set that shows a clear trend although there is gap in data between the pond fraction 0.15 and 0.25

[Printer-friendly version](#)[Discussion paper](#)

AR: We agree. Unfortunately, we do not have the data to fill the gap.

—

RC: P8 L1: “Therefore we need the brightness temperature at the ice surface...in summer...”. But don’t you need in other seasons as well? Or you assume zero water vapour in other seasons? Please justify the statement. But the methodology described in the rest of this paragraph is good.

AR: We apologize for the confusion and recognize the need for further clarification here. In other seasons, MEMLS computes the ice brightness temperature, which is then used directly in the atmospheric RTM, giving us the brightness temperature at the top of the atmosphere. For the summer ice brightness temperature, we use empirical data, which is measured at the top of the atmosphere. We therefore cannot feed this to the atmospheric RTM, because the atmosphere would be taken into account twice in this case. Instead, we want to come back to a value representing the ice surface brightness temperature that can be combined with the ocean brightness temperature, and then the atmospheric effect, in the RTM. This is why we apply this procedure in summer only. We will try to clarify.

—

RC: P8 Section 3.2: “Ocean is not covered by 100 % of sea ice”. Yes, but should mention that this is a more serious problem particularly with the coarse-resolution 6.9 GHz.

AR: We will clarify. However, the resolution of the satellite footprint is not an issue for the simulation of the brightness temperature of a mixed ice-ocean surface. It is rather the difference between model and satellite resolution in the later comparison that is a challenge.

—

RC: P9 L2: is there a name for the “Wentz and Meissner (2000)”? is it an original

model or modified from a previous model?

AR: It is an original model developed to support their "Ocean retrieval algorithm" for AMSR measurements. There is no specific name for this model as far as we know of.

—

RC: P9 L19: "but some characteristic features inherent to the mean model state might remain..." such as what? Also, can you comment on why the uncertainty of the observed brightness temperature itself is considered to be small? Nothing is mentioned in section 4.1.

AR: By "characteristic features inherent to the mean model state", we mean that the mean model state and the assimilated state are so incompatible that the model will drift towards mean model state if the assimilation step is too long. Let's say, for example, that the model has a warm bias in a given region. If a non-zero sea-ice concentration is assimilated into that region, the sea ice might melt completely until the next step because the ocean is too warm at that place for sea ice to exist. Such effects are minimized by assimilating several climate variables at the same time. However, more complex relationships might be at work which cannot be compensated by assimilating most of the variables from reanalysis. The uncertainty of AMSR measurements are estimated to be 1 K. This will be added to Section 4.1.

—

RC: P11 L11: "In order to allow for a realistic relation between ice concentration and thickness,...". I don't see an easy way to do this. In order to save reader's time on checking the given reference please describe in one line how this was done.

AR: As mentioned, the method is very simple and the reference shows that it outperforms other methods used for data assimilation of ice thickness. The assimilation changes the thickness in the given grid cell by  $\Delta h_{\text{assim}}$ , which is proportional to  $\Delta \text{SIC}_{\text{assim}}$ , with a proportionality factor  $h^*$  of 2m, as follows:  $\Delta h_{\text{assim}} = h^*$

Printer-friendly version

Discussion paper





ΔSIC\_assim We will add such description.

—

RC: P11 L25-29: The simulated Tb are slightly higher in regions of high ice concentration and thickness, and vice versa. How high and how low? Also, would you suggest reasons to explain this observation, especially when it is coming from the 3 runs? One would expect the difference to be small in winter season when the concentration approaches 100 %.

AR: We have investigated these differences from several perspectives, trying to link them with parameters used as input for ARC3O. The only explanation that can explain the pattern and magnitude for too high brightness temperatures in the regions North of Canada (by about 3 to 5 K) is the too low ice thickness in the simulation, likely induced by the assimilation process, as explained later in the manuscript. Regarding the regions of too low brightness temperatures, they are mainly marginal zones with seasonal ice cover, reaching an underestimation of 10 to 15 K in some cases. However, here, the too low brightness temperatures only occur when the brightness temperatures are simulated based on NASA Team or SICCI sea-ice concentrations, not when they are simulated based on the Bootstrap sea-ice concentrations. As can be seen in the last row of Fig. 8, the sea-ice concentration is much lower in these regions in the NT and SICCI product than in Bootstrap. These differences are therefore rather a result of differences in the retrieved sea-ice concentration than a problem in the simulation.

—

RC: P13 L3: “NASA Team brightness temperatures...” You mean brightness temperature from using NASA Team. Of course, NT does not produce Tb.

AR: Yes of course. We will correct that.

—

RC: P13 L17: you use only the SICCI2 run to examine the sensitivity and justify the

use of this single run, based on the fact that “physical relationships linking the different variables are the same in all three assimilation runs”. But would the different conceptual framework in different retrieval methods play a role here?

AR: Using different retrievals might have an influence on the magnitude of the variability of the different parameters used for the sensitivity study and therefore it may influence some of the spatial patterns of importance of different parameters. However, we do not think it would strongly influence our qualitative conclusions about the prevailing importance of sea-ice concentration in regions with less than 100 % sea-ice concentration and the prevailing importance of surface temperature in regions of 100 % sea-ice concentration. Again, this is because the sensitivity study highlights relationships of variability rather than relationships of mean states. We will however check this in our data to make sure, and will say so in the paper.

—  
RC: P13 L24-28: any suggested threshold on the ice concentration that causes switching the sensitivity from the concentration to the surface temperature? Do you think this is also linked with the ice type?

AR: We think the threshold where it switches to surface temperature is for sea-ice concentrations close to 100 %, with low variabilities throughout the years. This is because the sea-ice concentration has such a high influence in the other regions that the variability has to remain low to allow another parameter to have a higher influence. In numbers: Considering that water TB is around 160 K and ice TB is around 260 K, the sensitivity of the total Tb to changes in sea-ice concentration is close to 1 K per 1 % of sea-ice concentration. As the penetration depth at 6.9 GHz is high, the sensitivity of the total Tb to the surface temperature will be significantly less than 1K per 1K. We have not checked yet if this is linked to ice types but plan to look into it in future work.

—  
RC: P14 Table 1: this is an important contribution

[Printer-friendly version](#)[Discussion paper](#)

AR: Thank you.

—

RC: P14 L8: “data assimilation on sea ice concentration...” Is it “on” or should be “of”?

AR: It should be "on". We want to investigate the effect of the method of data assimilation on the sea-ice simulated by the model.

—

RC: P14 L10: the difference of concentration in the MIZ can be as large as, say, 30 % (not 5 %)

AR: We do not mean differences in sea-ice concentration between the observational products. Instead, we mean the difference between the observational product and the assimilated sea-ice concentration. With an ideal assimilation method and ideal model, the observed and the assimilated sea-ice concentration should be the same. Here, they are not, however, but the differences between the product and the assimilated ice only reach 5 % in marginal regions (see Fig. 6) We will rewrite the sentence to clarify.

—

RC: P15 last paragraph: but cannot you evaluate the difference for cases of 100 % ice concentration only? That would still be useful

AR: To do so, we would need to be sure that the location chosen have exactly the same properties, such as concentration, thickness, surface temperature in reality and the model at the same time, which is hard to do.

—

RC: P16 L2: the difference between the Bootstrap and NT algorithms varies depending on the ice cover and season. I am not sure that Bootstrap always give higher range. You quoted 2 references. Have you checked more sources?

[Printer-friendly version](#)

[Discussion paper](#)



AR: We understand your concern and will provide further references.

—

RC: P17 L6: I think 2 m ice thickness is reasonable assumption. 4 m is too much. Please confirm this 4 m by quoting a reference

AR: Our use of 4 m was to check if the ice thickness can have such an impact on the brightness temperature. The assumption of around 4 m thickness of the thickest ice north of Greenland and the Canadian Archipelago is based on the new SMOS/CryoSat2 product (see Fig.9 in Ricker et al., 2017, The Cryosphere or the quick-look: <https://spaces.awi.de/pages/viewpage.action?pageId=291898639>). The product shows rather a thickness of between 3 and 4 m. We will clarify.

—

RC: P17 last paragraph: the assumption of a cell having one ice types (MY ice if ice keep circulating for more than a year) is difficult to accept. You hardly find ice circulating within one cell for more than a year. With the very large cell dimension from the 6.9 GHz observations, the cell is almost always heterogeneous (MY, FY ice and OW) in highly dynamic regions such as the Beaufort Sea. I would suggest reconsidering this a possible source of error.

AR: We agree and will highlight this issue further.

—

RC: P19 L25: “As” instead of “Like”

AR: Changed.

—

RC: P19 L28: when include several references between brackets it is preferable to order them from old to recent)

Printer-friendly version

Discussion paper



AR: Noted, we will check the manuscript for such occurrences.

—

RC: P19 L34: the sentence is not clear. Please rephrase.

AR: Noted, we will reformulate.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-318>, 2020.

TCD

Interactive  
comment

Printer-friendly version

Discussion paper

