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

Anonymous Referee #1

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Review of manuscript "The Arctic Ocean Observation Operator for 6.9 GHz (ARC3O) - Part 1: development and evaluation"

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 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.

Comments to be considered by the authors:

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.

P 1 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?

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

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.

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

P2 L24: the promise made in the statement “While we focus on the frequency of 6.9 GHz 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.

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

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?

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?

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

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?

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.

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

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.

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.

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.

P9 L2: is there a name for the "Wentz and Meissner (2000)"? is it an original model of modified from a previous model?

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.

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.

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%.

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

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?

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?

P14 Table 1: this is an important contribution

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

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

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

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?

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.

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 heterogenous (MY, FY ice and OW) in highly dynamic regions such as the Beaufort Sea. I would suggest reconsidering this a possible source of error.

P19 L25: “As” instead of “Like” P19 L28: when include several references between brackets it is preferable to order them from old to recent) P19 L34: the sentence is not clear. Please rephrase.

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