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The Cryosphere
Discussions

Interactive comment on "The Arctic Ocean Observation Operator for 6.9 GHz (ARC3O) – Part 1: How to obtain sea-ice brightness temperatures at 6.9 GHz from climate model output" *by* Clara Burgard et al.

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RC: Reviewer Comment, AR: Author Response

RC: Reviewer summary: The authors consider the development of an observation operator to provide passive microwave brightness data at 6.9 GHz frequency and Vertical polarization. The work is motivated by the need to overcome observational uncertainty introduced by geophysical retrieval algorithms applied to satellite observations and used to initialize and evaluate climate models. Here, the observation operator

simulates the brightness temperature from the climate model output instead of requiring the retrieved sea ice concentration from observed brightness temperature data. Consideration of the feasibility and limitations of the observation operator concept for simulated sea ice is the main focus here. The authors use highly resolved 1D thermodynamic sea-ice and 1D microwave emission models to consider the effect that the simplified temperature and salinity profiles characteristic of GCM outputs have on brightness temperature estimates and observation operator performance. Generally, the approach works well for cold, winter conditions, and in the peak of summer when surface melt ponds are present, but not during periods of wet snow. The authors determine the boundary conditions for the construction of an operator that is evaluated against satellite brightness temperatures in their companion paper (which I did not evaluate). In general the paper is well written and the descriptions and figures are mostly clear and concise. Appendix A is useful for providing equations though Appendix B is just a table that could be in the paper. The methods should be better organized, and made to be distinct from the results, to make the paper easier to follow. For example, on Page 10, around line 11, there are new methods and their reasoning described in amongst the section focused on the results presented in Figure 3. The authors should clarify their positioning on the role that snow plays on the examined 6.9 GHz frequency and vertical polarization, in the contexts of season, ice type, and other available frequencies and polarizations. It is mostly all there, just hard to follow. For example, the negligible contribution of dry snow properties compared to ice (due to brine in the ice) is cited is advantageous for the $\sim$4.3 cm wavelength examined, yet there is a section looking into the role of dry snow (Section 6) and the following statement is made "the radiative effect of the snow cover hence remains important.". At the beginning of Section 7.3 snow is cited as a limiting factor. Perhaps it is better to make it clearer earlier in the paper that one of the goals of the study is to better understand the potential impact of dry (and wet snow) conditions on the operator output. Statements about wet snow are easier to follow as there are not contradictions.

AR: Thank you very much for the positive feedback, and for your detailed, constructive comments on how to further improve our paper. We will work on a new clearer structure for the manuscript. Also we plan to clarify the issue of snow for our study further. We plan to address the other comments as described in the following.

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RC: P3L22: 'atmosphere' doesn't fit here because the sentence is referring to sea ice.

AR: Thank you for pointing that out. We will reformulate the sentence to clarify that we are describing the brightness temperature measured by the satellite from space.

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RC: P5L7-10: The purpose behind defining specific locations is unclear. This is especially true since the authors indicate that sea ice seldom exists at the first-year sea ice location. The choice of locations for the sensitivity analysis are also arbitrary. If the choice of location does not affect the study then the locational context isn't needed.

AR: We will follow your suggestion and try to describe the forcing data in a more conceptual way.

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RC: P7: The paragraph on the bottom, beginning "Our input for the emission model...", is somewhat dismissive of the breadth of in-situ observations that are available, and the role of these observations in model development. It would be clearer is the authors outlined the model set-up, inputs, and assumptions used, since this is a methods section, and save uncertainty evaluations and suggestions for the discussion section.

AR: We agree that it is more common to discuss uncertainties after presenting the results. However, in this case, we want to make clear to the reader right in the beginning that, while there might be many uncertainties, they do not affect our results substantially. This way, the reader can concentrate on our results without being concerned about these limitations while reading the paper.

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RC: P9L8: Is it correct to say that April in the Arctic is summer?

AR: We apologize for the confusion. To be more precise, we will change "summer" to "warm conditions" and "winter" to "cold conditions".

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RC: P9 Figure 3: Symbols for FYI and MYI are not clear in the figure.

AR: Thank you for pointing that out. We will work on another for differences between FYI and MYI.

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RC: P14L3: It is confusing that the assumption of constant salinity introduces large uncertainties in the brightness temperature during summer, when earlier the authors mentioned the properties inside the ice do not influence the brightness temperature when the ice surface has a brine volume fraction higher than 0.2 (also during summer). Also on P15 (L7-8) the authors say the brightness temperature depends on the surface rather than internal ice properties. Some clarification given in the context of expected penetration depth would be helpful.

AR: We apologize for the confusion. We will clarify this.

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RC: P16L15-17: Indicate what would happen if ice concentration were <100 %.

AR: Here, we check the effect of the atmosphere on the brightness temperature over a sea-ice surface. The radiative transfer model we use to compute the atmospheric influence on the brightness temperature also includes the computation of the ocean surface brightness temperature. There is no possibility of separating the two and using the atmospheric module alone. As we are solely interested in the brightness temperature above ice surfaces in this study, we therefore have to assume 100 % sea-ice concentration as input to this radiative transfer model. Using an ice concentration smaller than 100 % would mean having the effect of the ocean surface on the brightness temperature as well.

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RC: P16L18: Section 7 should be "Discussion and Conclusion".

AR: We acknowledge that this would be a more typical way of structuring the manuscript. However, we prefer to keep Section 8 as a short conclusion and leave Section 7 to a Summary and Discussion. However, we will rethink Section 7 in the process of restructuring the manuscript.

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RC: P17L19-20: Sentence "In summer..." is confusing i.e. how is the liquid water fraction highly sensitive to changes in salinity. Do you mean the salinity of the melt ponds?

AR: We apologize for the use of "liquid water fraction" here, we actually mean "brine volume fraction". We will reformulate to avoid confusion. The brine volume fraction is highly sensitive to changes in bulk salinity and temperature. As temperatures are near 0°C, ice can only exist at very low salinities. The brine volume fraction increases very fast for low brine salinities but salinities of 1 g/kg or even more, following Eq. A4.

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RC: P18L33: The authors should elaborate on how the brightness temperature would be weighted by melt pond fraction.

AR: We will clarify our suggestion for this process.

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RC: P19L3: How would periods of wet snow be identified?

AR: Here also, we will clarify our suggestion for this process.

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RC: P19 Appendix A: Indicate the validity ranges of the formulas.

AR: We are sorry if this is not clear. We will add the validity ranges.

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